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Annual Book Issue

The Significance of Physical Theory: *Victor F. Lenzen* 517

Recommended Diet for Padded Writing: *Herman R. Struck* 522

Does Writing Make an Exact Man? *Eugene S. McCartney* 525

Conference on Scientific Editorial Problems

Introduction: *Marian Fineman* 529

Probable Trends in Scientific Publications as Viewed from the Editor's Office: *W. Albert Noyes, Jr.* 529

Problems in Financial Management of Scientific Journals: *Milton O. Lee* 530

Problems of the Editor of a Small Journal: *George S. Tulloch* 532

Illustrations for Scientific Publications: *Ruth C. Christman* 534

Jargon—Good and Bad: *Joseph D. Elder* 536

Publishing as Applied Science: *Ralph B. Smith* 538

News and Notes 539

Book Reviews

General 544

Astronomy and Mathematics 546

Physics 550

Chemistry and Biochemistry 553

Biology and Biophysics 556

The Medical Sciences 562

Communications

Can Scientists Write for the General Public? *Florence Moog* 567

Meter Versus Yard: *Dennis Henkel* 567

Book Review Index 568

The Scientific Author as I Have Known Him 3A

Meetings & Conferences 34A

AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE

The Significance of Physical Theory

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I

THE present century has witnessed exceptional activity in the construction of theories for various fields of physical science. Theories of relativity and of the structure of atoms constitute impressive monuments to man's creative activity in his quest for rational knowledge of the natural world. Philosophic reflection concerning the character of physical theory has run parallel to scientific creation. Among classics of the philosophy of theoretical physics is the book of Pierre Duhem, *La Théorie Physique; Son Object, Sa Structure*, the first edition of which was published in 1906, and the second in 1914. The present occasion for discussion of Duhem's conception of physical theory is the publication, by the Princeton University Press, of an English translation of the just-cited work by Phillip P. Wiener. The American edition bears the title THE AIM AND STRUCTURE OF PHYSICAL THEORY and includes an informative foreword, "Pierre Duhem's Life and Work," by Louis de Broglie, Nobel laureate and discoverer of the wave properties of matter (*1*).

A physical theory is a rational construction that expresses in systematic form man's knowledge of the physical world. Albert Einstein once introduced an exposition of the theory of relativity by a discussion of the nature of physical theories (*2*). As foundation for his analysis, he distinguished between constructive theories and theories of principle.

A *constructive theory* expresses the attempt to build a picture of complex phenomena out of relatively simple constituents. The kinetic theory of gases, for example, reduces mechanical, thermal, and diffusional properties of gases to molecular motions. Einstein declared that when we say that we understand a group of natural phenomena, we mean that we have found a constructive theory which embraces them.

A *theory of principle*, or *abstractive theory*, requires not the synthetic but the analytic method. Procedure for theories of principle begins with abstraction from experience instead of construction of hypothetical elements. Thermodynamics, for example, is based upon abstract principles which express the generalization that perpetual motion never occurs in ordinary experience.

According to Einstein, the merit of constructive theories is their comprehensiveness, adaptability, and clarity; that of theories of principle is their logical perfection and security of foundations.

In the present era of physical science, constructive theories of molecules, atoms, and nuclei command the universal interest of physicists and chemists. But it has not always been so. At the beginning of the 20th century, the doctrine of energetics found in thermodynamics the ideal form of physical theory. Among the proponents of energetics was the eminent French theoretical physicist, Pierre Duhem. According to the latter, constructive theories, designed to explain physical phenomena, are to be rejected as metaphysics. He summarized his doctrine of physical theory in the statement:

> A physical theory is not an explanation; it is a system of mathematical propositions whose aim is to represent as simply, as completely, and as exactly as possible a whole group of experimental laws.

In other words, Duhem held that only theories of principle, or abstractive theories, should be accepted for physical theory.

In the preface to the second edition of *La Théorie Physique*, Duhem held that his conception of theoretical physics had been confirmed by developments. This claim was supported by the creation of the theory of relativity which Einstein characterized in the aforementioned discussion as a theory of principle. But atomism, which Duhem vigorously opposed, had already begun the extraordinary developments that have made constructive theories of atom and nucleus the focus of contemporary physical interest. Thus, the course of physical theory has not been limited by the aims described by Duhem. Nevertheless, philosophers and historians of science will welcome the appearance of the excellent English translation of his significant book. Contemporary philosophy of science has emphasized the role of theoretical construction in physical theory, and Duhem's work provides an excellent introduction to this point of view.

The theory of Duhem was in its essentials an important contribution to a critical attitude on the part of physicists toward their subject. The best-known of these critical physicists was perhaps Ernst Mach, physicist, historian and philosopher of science, whose activities extended from the second half of the 19th century into the early decades of the 20th century. The aim of Mach was to eliminate metaphysics from science; to that end he described science as the economical description of the facts of experience (*3*). Since molecules and atoms were conceived to lie beyond the possibility of direct perception, Mach banned them from physical theory as fictions. Mach exerted a profound influence and his antimetaphysical doctrine was continued by the Vienna Circle, the ideas of which have been expounded in the United States by Philipp Frank (*4*). The Central European successors of Mach did diverge from him in that they accepted atoms as useful constructs. On this issue, however, Duhem's conception of physical theory con-

formed to that of Mach. Referring to the great development of molecular theories and mechanical models, Duhem said:

> Toward the end of the nineteenth century, hypothetical theories which were offered as more or less probable explanations of phenomena were extraordinarily multiplied. The noise of their battles and the fracas of their collapse have wearied physicists and led them gradually back to the sound doctrines Newton had expressed so forcefully. Renewing the interrupted tradition, Ernst Mach has defined theoretical physics as an abstract and condensed representation of natural phenomena.

While Duhem adhered to Mach's view that physical theory is an autonomous, abstract, and economical representation of physical phenomena, he did not share the expressed antimetaphysical attitude of Mach. Duhem left a place for an independent metaphysics of reality. For Mach and his followers, objects of perception are constructs that correlate the data of sense; Duhem had a less critical idea of the perceived object and accepted its reality from common sense. Whereas Mach denied the significance of the concept of transcendent reality, Duhem spoke of reality behind appearance. He thus retained the traditional dualism between reality and appearance, between metaphysics and physics. Thus he said:

> What is this metaphysical affirmation that the physicist will make, despite the nearly forced restraint imposed on the method he customarily uses? He will affirm that underneath the observable data, the only data accessible to his methods of study, are hidden realities whose essence cannot be grasped by these same methods, and that these realities are arranged in a certain order which physical science cannot directly contemplate. But he will note that physical theory through its successive advances tends to arrange experimental laws in an order more and more analogous to the transcendent order according to which the realities are classified, that as a result physical theory advances gradually toward its limiting form, namely, that of a *natural classification*, and finally that logical unity is a characteristic without which physical theory cannot claim this rank of a natural classification.

II

In order to set forth the historical significance of Duhem's work, I shall sketch the philosophic background of his ideas concerning the object of physical theory. A characteristic attitude was his rejection of atomism for the explanation of physical phenomena. Now atomism was especially developed by the ancient Greek philosopher Democritus, who taught that reality is constituted of atoms in the void. He held that properties of atoms, such as figure and motion, alone are real and that sensible qualities, such as color, hotness, and sweetness, are appearances which are to be explained by the action of atoms upon the organs of sense. Thus originated the doctrine that spatial and mechanical properties of matter are fundamental and constitute the object of physical theory. In modern terminology, the basic properties of matter are primary and real; sensible qualities are secondary qualities with an inferior status in reality.

The ancients accepted earth, air, fire, and water as fundamental elements, and Plato in the *Timaeus* expounded a theory that constituted the elements out of geometric figures. This Platonic reduction of quality to quantity was the initiation of a mathematical physics, but it was thrust aside by the Aristotelian physics which recognized the qualities of hotness and coldness, of wetness and dryness as fundamental. It was the historic function of Galileo to renounce the Aristotelian program of a qualitative physics and to found a quantitative science based upon the primacy of spatial and inertial properties of physical reality to the secondary qualities of sensation. Whether matter was interpreted as continuous space by Descartes, or as atoms in empty space by the atomists, primary properties were sharply distinguished from secondary qualities and made the object of physical theory.

The distinction between primary qualities and secondary qualities was adopted in the 17th century by the founders of modern physics such as Galileo, Descartes, and Newton. The distinction is exemplified by the contrast between the sensation of light and the physical action of external bodies upon the sense organs of an observer. This distinction between sensible appearance and physical cause was generally accepted by natural scientists and found philosophic acceptance in Locke's *Essay Concerning Human Understanding* (1690). The history of modern philosophy is largely an attempt to overcome this dualistic realism of the physicists. The philosopher Berkeley found that primary qualities, as well as secondary ones, are infected by relativity to the observer. Hume's skeptical analysis yielded the result that physical objects are complexes of sensory impressions. The philosopher Kant held that space and time are forms of intuition and that physical objects are constructions of thought out of data given in intuition. But, in the last analysis, Kant adhered to the original dualistic realism, for he attributed the manifold given in intuition to the action of a thing-in-itself.

Against a background of dualistic realism presupposed by natural scientists, there appeared the analysis of scientific knowledge by Ernst Mach. In order to eliminate metaphysics from science, he analyzed reality into elements of sensation which are neither physical nor mental by themselves. The things given in perception are complexes of sensible elements and are viewed as real. Insofar as properties of things are determined by other things, they are physical; but insofar as properties are related to an observing organism, they are mental. Thus, all sciences refer to the same content; in our time, this doctrine has furnished the basis of the movement for the Unity of Science (*5*).

The neutral character ascribed by Mach to the elements of sensation stimulated the creation of a Neorealism during the early decades of the 20th century, a movement that was part of what Lovejoy has called

the revolt against dualism (*6*). In Europe, the Vienna Circle, under the leadership of Moritz Schlick and Rudolf Carnap, developed the doctrine of Mach that was transplanted to America under the name *logical positivism*. The program of positivism was to give an interpretation of science that is neutral with respect to metaphysical issues. As set forth by Philipp Frank, Mach's doctrine is that scientific knowledge refers only to the contents of perception. Science consists in the symbolic representation of perceptions. In departure from Mach, however, contemporary positivists accept molecules and atoms as constructs that play a valuable role in the symbolic representation of physical reality.

The neutralist interpretation of Mach's doctrine has manifested instability. The usual philosophic opinion ascribes transient reality to contents of perception. Hence, if reality consists of elements of sensation, it is dependent upon intermittent acts of perception. In view of this apparent logical consequence of positivist criticism, the doctrine of dualistic realism has retained adherents. Thus Max Planck, who wrote frequently on philosophic problems of physics, reported that in his early period he had adhered to the doctrine of Mach, but that he finally accepted the realistic point of view (*7*).

Bertrand Russell began as dualist; then in *Scientific Method in Philosophy* he described physical objects as logical constructions out of aspects given in perception (*8*); but later in the *Analysis of Matter* he expounded a causal theory of perception, a form of dualistic realism (*9*). The history of the theory of perceptible things, thus, narrates a vacillation between the characterization of a thing as constituted of transient sense data and as a persistent reality that gives rise to the momentary content of perception.

Duhem's theory was anomalous within a modern context. Central to this conception of physical theory was the rejection of the program to reduce quality to quantity. He stood with Aristotle against atomists and Cartesians by assigning a fundamental role to quality as a direct object of physical investigation. Duhem has been classified as an adherent of the school of Mach, but he did not adhere to the latter's apparent restriction of reality to the data of experience. Duhem proclaimed the autonomy of physical theory with respect to metaphysics but declared that there are realities which transcend experience. Thus he said:

> Concerning the very nature of things, or the realities hidden under the phenomena we are studying, a theory conceived on the plan we have just drawn teaches us absolutely nothing.

III

According to Duhem, the aim of physical theory is not explanation but the representation and natural classification of experimental laws. The sequence of procedures is as follows: Physical properties of things are represented by symbols, hypotheses are set up to express relationships between symbols, consequences are deduced mathematically from hypotheses, and the consequences are then tested by experimental facts. For Duhem, the abstract character of theories of principle expresses the nature of physical theory. But such abstractive theories are formulated in terms of abstract symbols which substitute for the concrete data of experiments. The laws of physics are symbolic relationships, each of which is approximate and provisional.

Duhem's conception of physical theory rests upon a distinction between practical fact and theoretical fact. Propositions about the properties of things given to observation express practical facts. Though vague and indefinite, such propositions based upon direct experience are either true or false. Theory, however, applies to a schematic construction. Through measurement, an observable property is correlated to a property of the symbolic schema. Propositions of a physical theory express theoretical facts about the schema. Though clear and definite, theoretical propositions are neither true nor false. In view of the lack of precision of measurement, an infinity of theoretical facts may be consistent with the results of experiment. A physical theory is approximate and provisional.

"An experiment in physics is the precise observation of phenomena accompanied by an interpretation of these phenomena; this interpretation substitutes for the concrete data really gathered by observation abstract and symbolic representations which correspond to them by virtue of the theories admitted by the observer." It is the theoretical interpretation of experiments through abstract and symbolic judgments that makes possible the use of instruments. Whereas the contemporary exponent of operationism interprets theory in terms of experiment, Duhem interpreted experiment in terms of theory. Duhem, thus, in effect continued the Kantian view that thought is constitutive of science. For illustration he offered the following example:

> Go into this laboratory; draw near this table crowded with so much apparatus: an electric battery, copper wire wrapped in silk, vessels filled with mercury, coils, a small iron bar carrying a mirror. An observer plunges the metallic stem of a rod, mounted with rubber, into small holes; the iron oscillates and, by means of the mirror tied to it, sends a beam of light over a celluloid ruler, and the observer follows the movement of the light beam on it. There, no doubt, you have an experiment; by means of the vibration of this spot of light, this physicist minutely observes the oscillations of the piece of iron. Ask him now what he is doing. Is he going to answer: "I am studying the oscillations of the piece of iron carrying this mirror?" No, he will tell you that he is measuring the electrical resistance of a coil. If you are astonished and ask him what meaning these words have, and what relation they have to the phenomena he has perceived and which you have at the same time perceived, he will reply that your question would require some very long explanations, and he will recommend that you take a course in electricity.

Duhem summarized his discussion by the statement:

> The result of the operations in which an experimental physicist is engaged is by no means the perception

of a group of concrete facts; it is the formulation of a judgment interrelating certain abstract and symbolic ideas which theories alone correlate to the facts really observed.

An interpretation of Duhem's conception of physical theory requires careful consideration of the reference of symbols. The physical properties of things are represented by numerical measures, but the basic question concerns the properties that the numbers represent. What are the abstract and symbolic ideas which theories alone correlate to the facts really observed? The adherents of Ernst Mach would declare that the symbols designate concrete experiences of spaces, times, colors, pressures, sounds, and so forth. The exponents of contemporary operationism would assert that the symbols designate operations by which the results of measurements are obtained (*10*). According to the well-developed views of Henry Margenau, the symbols designate constructs (*11*). An interpretation of Duhem may be determined from the following:

> When, in the course of an optical theory, we talk about luminous vibration, we no longer think of a real to-and-fro motion of a real body; we imagine only an abstract magnitude, *i.e.*, a pure, geometrical expression. It is a periodically variable length which helps us state the hypotheses of optics, and to regain by regular calculations the experimental laws governing light. This vibration is to our mind a *representation*, and not an *explanation*.

From this example, one may conclude that for Duhem the symbols of physical theory designate constructs in the contemporary sense of the term. Duhem, however, declined to accept molecules, atoms, and electrons as legitimate constructs. Thus, his point of view appears more limited than contemporary theories of physical constructs.

Duhem rejected all mechanical explanations of physical phenomena. Thus, he criticized the Cartesian theory of vortices and pressures in extended matter and especially disapproved of the contemporary development of atomic theories. Thus, he wrote:

> Consider someone, for instance, who would take physical theory just as we have it, in the year of grace 1905, presented by the majority of those who teach it. Anyone who would listen closely to the talk of classes and to the gossip of the laboratories without looking back or caring for what used to be taught, would hear physicists constantly employing in their theories molecules, atoms, and electrons, counting these small bodies and determining their size, their mass, their charge. . . . each time the fortunate daring of an experimenter will have discovered a new set of experimental laws, he will see the atomists, with feverish haste, take possession of this scarcely explored domain and construct a mechanism approximately representing these new findings. Then, as the experimenter's discoveries become more numerous and detailed, he will see the atomist's combinations get complicated, disturbed, overburdened with arbitrary complications without succeeding, however, in rendering a precise account of the new laws or in connecting them solidly in the old laws. . . . It will appear clearly to him that the physics of atomism, condemned to perpetual fresh starts, does not tend by continued progress to the ideal form of physical theory; whereas he will surmise the gradually complete realization of this ideal when he contemplates the development which abstract theory has undergone from Scholasticism to Galileo and Descartes; from Huygens, Leibniz and Newton to D'Alembert, Euler, Laplace, and Lagrange; from Sadi Carnot and Clausius to Gibbs and Helmholtz.

Duhem denied that the nature of ultimate reality is the object of physical theory. But he committed himself to a reality beyond experience more explicitly than positivists with whom he has been classified. Thus,

> Physical theory never gives us the explanation of experimental laws; it never reveals realities hiding under the sensible appearances; but the more complete it becomes, the more we apprehend that the logical order in which theory orders experimental laws is the reflection of an ontological order, the more we suspect that the relations it establishes among the data of observation correspond to real relations among things, and the more we feel that theory tends to be a natural classification.

IV

It is evident from Duhem's symbolic conception of physical theory that hypotheses are not directly derivable from experience. Accordingly, he expressed disagreement with Newton's doctrine that the principles of a physical theory should be derived by induction from experience. Newton's theory of scientific method was expounded in the "General Scholium" of his *Principia*, but it was also stated in Query XXXI at the end of the second edition of the *Optics*. In the passage quoted by Duhem, Newton stated:

> To tell us that every species of things is endowed with an occult specific quality by which it acts and produces manifest effects, is to tell us nothing; but to derive two or three general principles of motion from phenomena, and afterwards to tell us how the properties and actions of all corporeal things follow from those manifest principles, would be a very great step in philosophy.

Duhem remarked:

> It was this sort of physical theory that Newton had in mind when, in the "General Scholium" which crowns his *Principia*, he [Newton] rejected so vigorously as outside of natural philosophy any hypothesis that induction did not extract from experiment; when he asserted that in a sound physics every proposition should be drawn from phenomena and generalized by induction.

It has been held that Newton derived his theory of gravitation from the laws which were revealed to Kepler by observation. Duhem declared, however,

> The principle of universal gravity, very far from being derivable by generalization and induction from the observational laws of Kepler, formally contradicts these laws. If Newton's theory is correct, Kepler's laws are necessarily false.

The fact is that the law of gravitation determines the

force acting on a planet to be the resultant of the attractive forces exerted by the sun and the other planets. In consequence, the actual orbit arises as a perturbation of the Keplerian orbit, which perturbation can be determined from refined observations. And Duhem further declared:

> Such a comparison will not only bear on this or that part of the Newtonian principle, but will involve all its parts at the same time; with those it will also involve the principles of dynamics; besides, it will call in the aid of all the propositions of optics, the statics of gases, and the theory of heat, which are necessary to justify the properties of telescopes in their construction, regulation, and correction, and in the elimination of the errors caused by diurnal or annual aberration and by atmospheric refraction. It is no longer a matter of taking, one by one, laws justified by observation, and raising each of them by induction and generalization to the rank of a principle; it is a matter of comparing the corollaries of a whole group of hypotheses to a whole group of facts.

Duhem further criticized the Newtonian method in a discussion of Ampere's claim that he deduced his mathematical theory of electrodynamic phenomena only from experiment. Duhem concluded:

> Two rocky reefs make the purely inductive course inpracticable for the physicist. In the first place, no experimental law can serve the theorist before it has undergone an interpretation transforming it into a symbolic law; and this interpretation implies adherence to a whole set of theories. In the second place, no experimental law is exact but only approximate, and is therefore susceptible to an infinity of distinct symbolic translations; and among all these translations the physicist has to choose one which will provide him with a fruitful hypothesis without his choice being guided by experiment at all.

It follows from Duhem's doctrine that an individual hypothesis cannot be tested experimentally and that crucial experiments are not possible. Regarding the latter point, he gave a profound criticism of the generally held view that Foucault's experiment on the velocity of light in liquids demonstrated the wave theory.

V

Duhem was a practicing theoretical physicist, but he was also a historian of science of the first rank. His works on the history of mechanics, on Leonardo da Vinci, and on systems of the world from Plato to Copernicus are monuments of learning. Hence, one of the merits of Duhem's work that especially justifies this American edition is the wealth of historical matetrial that bears on physics. In order to illustrate his distinction between explanatory and abstract theories, he borrowed from Pascal a contrast between two types of mind. The broad but weak mind finds explanatory theories congenial to it; the narrow but strong mind delights in abstract theories. In French thought, Gassendi, the atomist, represents the first type, and Descartes, who sought to found physics on rational principles, represents the second type. But Duhem offered Continental physicists generally as examples of the narrow but strong mind. Newton was conceded to exemplify the narrow and strong type among the English, but Duhem found English physicists of the 19th century mainly to have broad and weak minds concerned with all kinds of mechanical models. Although Duhem supported his thesis with a wealth of historical material, nationalistic differences are hardly discernible in the contemporary international development of constructive hypotheses concerning atoms and nuclei. The French physicist Louis de Broglie quite rightly calls attention to the British physicist Dirac, whose work on the foundations of quantum mechanics has been conducted on the highest level of abstraction.

It was a thesis of Duhem that "hypotheses are not the product of sudden creation, but the result of progressive evolution." Thus, one cannot understand a hypothesis unless one knows its history. A valuable part of the book is the history of the concept of universal attraction from Aristotle to Newton. Duhem sums this up in the following:

> The most diverse considerations and the most disparate doctrines arose in turn to make their bid for the construction of celestial mechanics; common experience revealing gravity, as well as the scientific measurements of Tyco Brahe and of Picard; the observational laws formulated by Kepler, the vortices of the Cartesians and atomists, as well as the rational dynamics of Huygens; the metaphysical doctrines of the Aristotelians, as well as the systems of the physicians and the dreams of astrologers; comparisons of weight with magnetic action, as well as the affinities between the light and the mutual actions of heavenly bodies. In the course of this long and laborious birth, we can follow the slow and gradual transformations through which the theoretical system evolved; but at no time can we see a sudden and arbitrary creation of new hypotheses.

Duhem's historical point of view led him to novel evaluations of ancient doctrines. Although he proclaimed the autonomy of physical theory, and distinguished physical theory from a metaphysical cosmology, he asserted that the ideal form toward which physical theory tends is analogous to cosmology. As we have seen, for Duhem, general thermodynamics most adequately exemplified the ideal form of physical theory. He held that general thermodynamics is analogous to the cosmology of Aristotelian physics. Aristotelian physics recognized the categories of quantity and quality, as does the thermodynamics with its symbols for extensive and intensive properties. Aristotle recognized qualitative change as well as motion, as does thermodynamics. One of the essential theories of Aristotle's cosmology is that of the natural place of the elements. Duhem found the essence of this theory to be that a perfect state of the universe would be one of stable equilibrium, to which the world would return by natural motions if disturbed. But thermodynamics analogously conceives that isolated systems tend to a state of equilibrium in which entropy is a maximum. Duhem arrived at the remarkable conclusion:

If we rid the physics of Aristotle and of Scholasticism of the outworn and demoded scientific clothing covering it, and if we bring out in its vigorous and harmonious nakedness the living flesh of this cosmology, we would be struck by its resemblance to our modern physical theory.

At the close of the final article, "The Value of Physical Theory." Duhem summarized his qualified positivism. He said:

> the physicist is compelled to recognize that *it would be unreasonable to work for the progress of physical theory if this theory were not the increasingly better defined and more precise reflection of a metaphysics; the belief in an order transcending physics is the sole justification of physical theory.*

Duhem expressed the previously cited vacillations of the physicist with respect to the foregoing affirmation by a quotation from Pascal, whose philosophic spirit permeates Duhem's book, and who may be permitted to speak in his native tongue:

> Nous avons une impuissance de prouver invincible à tout le Dogmatisme; nous avons une idée de la verité invincible à toute le Pyrrhonisme.

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Recommended Diet for Padded Writing

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FROM a close examination of the writing in many scientific publications (including, if you do not mind, *The Scientific Monthly*), one would never guess that as a nation we are renowned for efficiency. Sentences bulge like overfed matrons with unnecessary words that obscure a writer's ideas and weaken his emphasis, much as the matronly fat obscures the streamlined glory of the past. For both matrons and sentences, a major solution is diet: for writing, a diet of efficient verbs.

The following sentences from *The Scientific Monthly* illustrate the point:

> Whereas [Cannon's] studies have been primarily concerned with the physiological regulations of the internal environment, much of the work of Richter has dealt with the maintenance of the constancy of the internal environment through the operation of behavior regulators.
>
> It is noted by Harrow that pancreatectomy is fatal to the dog, with the death of the animal occurring in one to two weeks and that the length of survival of cats after removal of the pancreas is about five to six days.
>
> Still more unusual is the fact that these surrounding industrial regions give relatively little employment to "Mainliners."

Shorter, clearer, and more forceful versions of these:

> . . . Richter has chiefly studied how behavior regulators maintain the constancy of the internal environment.
>
> Harrow notes that pancreatectomy is fatal to dogs in one to two weeks and fatal to cats in about five to six days.
>
> Still more unusual, these surrounding industrial regions employ relatively few "Mainliners."

Since readers may object, with some justification, to criticism of sentences removed from context, here is a complete—and representative—paragraph, again from *The Scientific Monthly*:

> The decision to stay on a job or leave it, as well as where to work, generally lay with the scientists themselves. Only 18 of the 155 scientists who had remained on their jobs for at least 8 years reported that they had had no other offer or none worth considering during this period. Only 67 of the 574 job exits were due to factors over which the scientists had no control, and 28 of these resulted from the termination of war projects. Furthermore, the scientists were rarely forced to accept a job for lack of another offer; this was the case for only 75 of the 670 job entrances covered by the study. Very likely, the fact that the scientists were able to choose between job offers was at least in part due to their practice of continuing in a position while shopping for a new one: they rarely left a job without having another one lined up.

In this paragraph, as a rewrite shows, more than 10 percent of the words are superfluous:

> The scientists themselves could generally decide where to work, and whether to stay on a job or leave it. Only 18 of the 155 scientists who had remained on their jobs for at least 8 years reported that they had had no other offer or none worth considering during this period. Factors over which the scientists had no control accounted for only 67 of the 574 job exits, and termination of war projects caused 28 of these. Furthermore, the scientists rarely had to accept a job for lack of another offer; among 670 job entrances, the study showed only 75 such cases. Very likely, the scientists could choose between job offers partly, at least, because they normally continued in one position while shopping for another: they rarely left a job without having a new one lined up.

Probably most readers will feel the increased crispness and clarity of the revision, particularly in the first and final sentences. The revision uses 137 words, against the original's 159.

If any contributor to *The Scientific Monthly* feels impelled at this point to mail a poisoned cake, I should like to remind him that I am not carping about a specific magazine or a specific group of writers. The same excesses appear in other publications as well:

> . . . it *has an opportunity to exercise an unobtrusive influence on* the course of science in the United States. . . . [A science review]
>
> . . . it can influence unobtrusively the course of science. . . .
>
> The fact that these teachers overlook *is that* the development of new interests is the job of the teacher. . . . [A book on education]
>
> These teachers overlook the fact that. . . .
>
> Finally, the Midwest was *in the process of swift change* during the nineteenth century. [A history of politics]
>
> . . . the Midwest was changing swiftly. . . .
>
> *There is* considerable work in the literature *which* supplies supporting, circumstantial evidence for the . . . hypothesis. . . . [A botany journal]
>
> Considerable work in the literature supplies. . . .

Such writing—wherever it appears—hampers the sharp, clear transfer of ideas. And certainly when a writer respects his ideas sufficiently to offer them up to cold print and the tough judgment of his colleagues, he wants to present them as cleanly as he can. This he can do more satisfactorily by forcing every verb to do its full duty. The verb, after all, is the spine of a sentence: when the verb falters, the sentence sags, because a less able word must carry the verb idea.

To use verbs efficiently, obviously a writer must first recognize superfluous verbs. This offers some difficulties, but a little practice helps enormously. Moreover, there are some useful guides.

1) Examine all forms of the verb *to be* (*am, is, are, was, were, be, being, been*). If a sentence contains the verb *to be*—and many sentences do—try converting one of the nouns to a verb. Examples (*1*):

> . . . but the experiment *was a complete failure.*
>
> failed completely
>
> . . . but its persistence *is the result of* two major developments.
>
> stems from
>
> . . . it *is of concern* also *to* scientists themselves.
>
> it also concerns scientists
>
> In all three types of decisions, *the considerations uppermost in the minds of* most of these scientists *were* the interest of the work, the earnings and opportunities it provided, and the working conditions on the job.
>
> most of these scientists considered primarily the interest of the work (*2*).

Occasionally, in reworking *to be*, the writer will find that converting a noun to a verb does not eliminate *to be* but merely reduces it to a helping verb. In such sentences, certain wordy constructions—often prepositional phrases—needlessly take over some of the verb's functions, as in a sentence quoted earlier: "Finally, the Midwest was in the process of swift change during the nineteenth century." Here, *in the process* suggests continuous action; *was* is the main verb. Use of the progressive (*was changing*) eliminates *in the process*, reduces *was* to an auxiliary, and stresses the notion of change.

The constructions *there is* (*there are, there were,* and so forth) and *it is* frequently employ a useless *to be*:

> For example, in recent years *there* has *been a tendency for* industry to "decentralize." . . .
>
> industry has tended to "decentralize"
>
> Although no figures were available for Daylesford, it is listed because *there is no doubt that* it is the least important stop on the route.
>
> because it is clearly the least important stop
>
> For some men, *it was* the opportunity to develop and grow with a problem *which* was most important.
>
> For some men, the opportunity to develop and grow with a problem was most important.

There is and *it is* require various cures. In the first of the foregoing sentences, for instance, a noun (*tendency*) becomes the main verb (*tended*), replacing *been*; in the second, an adverb (*clearly*) eliminates *there is*; in the third, *it was* and *which* are simply removed.

No one, incidentally, should condemn *to be*—or any other verb or construction that is cudgeled here—automatically. *To be,* to quote a good handbook, "is the most necessary single verb in the language" (*3*). But most of us overuse it. Furthermore, a hunt for *to be* synonyms (*exists, consists of*) profits nobody. *Some* word in the existing sentence must carry the

real verb idea, and the writer should search for that word; if the verb does not carry the idea, then a noun probably does (*4*).

2) Examine forms of *to have*. Again, look for a key noun. Examples:

> These developments *had a profound influence on* Philadelphia's Main Line District.
>
> These developments profoundly influenced
>
> Since these attitudes *have a decided influence on* behavior, information concerning them is valuable to employers, personnel workers, and scientists themselves.
>
> these attitudes strongly influence behavior
>
> . . . rats *have an ability to* make selections conducive to their well-being.
>
> rats can make selections

3) Look for verbs such as *give, make, do, occur, cause, effect, bring about*. Since a complete list of such verbs would be extremely long, finding them demands sharper eyes than finding *to be* forms. The verbs named are the chronic offenders, however, and a close inspection of nouns will enable a writer to identify the rest.

> Though the Catholic Church has never *given official approbation to* these art forms, they are tolerated. . . .
>
> has never officially approved
>
> A considerable number of scientists also *gave careful consideration to* the prospective working atmosphere in choosing a job.
>
> scientists also considered carefully
>
> . . . this *brought the end of* tolls.
>
> this ended tolls

A writer should not conclude that these verbs are always unnecessary. In "I gave him the book," *gave* or a synonym is the only verb possible. One must analyze the sentence, especially its nouns, to determine whether or not a particular verb carries the verb idea.

4) Finally, to rephrase a piece of advice that seems only slightly younger than the wheel but almost as useful: convert passive voice constructions that name the doer of the action to the active voice. In the passive voice, you may remember—if your grammar teacher harangued you dutifully, the subject is acted upon. A short sample: "their jobs were threatened by war-created circumstances." Using the doer of the action of subject produces a more brisk wording: "war-created circumstances threatened their jobs." Other examples:

> It is rather remarkable that this area has had such a limited development of industry when one considers that *it is surrounded by great industrial regions.*
>
> that great industrial regions surround it
>
> It was during this period that *a cult of the Egyptian Isis was introduced into this area by seagoing natives of Apuglia.*
>
> During this period, seagoing natives of Apuglia introduced into this area a cult of the Egyptian Isis. [This version also removes *it was* and *that.*]
>
> We should mention that *Isis-Horus have/sic/been depicted as black by the Egyptians.*
>
> that the Egyptians have depicted Isis-Horus as black.

A section from a paragraph shows how consistently this construction appears and how it can emasculate ideas (four of the five sentences contain superfluous passives):

> Several weeks after these symptoms had developed the animals were placed on the self-selection diet. It *was found* that a marked appetite for fat and olive oil *was shown* by the rats and they ate little or no carbohydrate, which in this experiment was sucrose. An increased appetite *was manifested* by all 7 animals for yeast. On the self-selection diet the diabetic symptoms of all the rats either disappeared or *were* greatly *reduced*. Upon the return to the McCollum diet, diabetic symptoms *were* again *shown* by 4 of the 7 experimental animals.

Without the italicized passives:

> Several weeks after these symptoms had developed, the animals were placed on the self-selection diet. The rats then showed a marked appetite for fat and olive oil and ate little or no carbohydrate, which in this experiment was sucrose. All 7 animals manifested an increased appetite for yeast. On the self-selection diet, the diabetic symptoms of all the rats disappeared or greatly decreased. Upon return to the McCollum diet, 4 of the 7 experimental animals again showed diabetic symptoms.

Since *manifested* in the third sentence is a general verb, and since the author uses *ate* in the preceding sentence, I cannot resist offering this version also, though the writer might scream "Distortion!": *All 7 animals ate more yeast.*

Writers who tend to overuse prepositional phrases should be especially wary of the passive, because it frequently requires another prepositional phrase. And an overdose of prepositional phrases can easily send a healthy reader to a sick bed. The last half of a sentence quoted earlier supports this statement reasonably well:

> . . . much *of* the work *of* Richter has dealt *with* the maintenance *of* the constancy *of* the internal environment *through* the operation *of* behavior regulators.

A hurrying reader would probably never thoroughly understand this clause; a careful reader would have to reread it. In fact, a linguistically inclined friend of mine considers this clause a monument to the infinite meanings of *of*. Successive *of* phrases, he declares enthusiastically, force a reader into horrifying mental gyrations. He points out that the five *of* phrases in the sentence are an old-fashioned grammarian's paradise; the first is a partitive genitive (*much of work*); the second, a subjective genitive (*Richter works*) or a possessive genitive (*Richter's*

work); the third, an objective genitive (*maintain constancy*); the fourth, a partitive again (he mumbled a little here about a descriptive in reverse); the fifth, a subjective genitive (*regulators operate*) with some possibility of being regarded also as an objective genitive (*operate regulators*). The human mind can stand just so much.

On a less learned plane, I am convinced that, with a few exceptions, a succession of three *of* phrases, or five of any kind, sets up a rocking-chair rhythm so inimical to ordinary prose that it destroys the reader's concentration on meaning. I first became aware of this fact several years ago when I was analyzing some 2000 revisions of various writers; since then I have seen no evidence that alters this view and I have seen a good deal that reinforces it. Several interesting *of* sentences, for instance, appear in Fowler and Fowler's *The King's English*:

> The signs of the times point to the necessity of the modification of the system of administration.
>
> The first private conference relating to the question of the convocation of representatives of the nation took place yesterday. (*5*)

The authors revise the first sentence to

> It is becoming clear that the administrative system must be modified.

And the second to

> The first private conference on national representation took place yesterday. (*5*)

Science writers' fascination for the passive is deplorable but understandable. For describing experiments, the passive (without the doer) sometimes performs even more efficiently than the active (*6*). Too, editorial demand for objectivity may force a writer into abandoning "I," which eventually leads him to "the writer," which eventually generates self-consciousness, which finally sends him slinking to the passive. Thus, editorial policy and the passive's efficiency in specific circumstances may develop in the writer a passive-psychosis, a state in which the patient cannot differentiate between a good passive and a bad one. However faulty this diagnosis, something certainly causes the disease, and to cure it a writer might well consider every passive sick until he proves it healthy.

To end this oracle-like piece realistically, I must admit that the preceding facts, even if heeded, will not guarantee entry into prose heaven. On the other hand, they do identify certain major snares and temptations along the way.

References and Notes

1. All examples, unless a source is given, are from *The Scientific Monthly*. For obvious reasons, I am not citing the titles of articles or the issue, but I will supply this information to anyone who is interested.
2. A minor comment: *the* (before *working*) and *on the job* are unnecessary.
3. Robert J. Geist and Richard Summers, *Current English Composition* (Rinehart, New York, 1951), p. 419.
4. Occasionally an adjective will contain the verb idea: "Clovers and alfalfa *have a greater beneficial effect* on the soil than any of the other legumes." (benefit the soil more) However, the construction seems rare. This example comes from a student paper.
5. H. W. Fowler and F. G. Fowler, *The King's English* (Oxford Univ. Press, London, 3rd ed., 1931), p. 15.
6. This statement does not mean, incidentally, that the passive minus the doer can do no evil; on the contrary, an unscrupulous or careless writer can easily use it to retreat into remoteness with an unsubstantiated "It is generally thought that . . .," a pronouncement that a comatose reader may accept without the quiver of a brain cell. But to my lay eye, science researchers do not take refuge in the passive unscrupulously.

Does Writing Make an Exact Man?

Eugene S. McCartney

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IN some parts of the world nothing brings the malign effects of the evil eye upon persons and things more quickly than perfection, or surpassing excellence, or even unqualified praise of them. A somewhat similar idea used to flourish among the Navajo women, who averted ill luck by weaving intentional imperfections into their rugs. Authors and editors do not have to take such precautions, for blemishes defy the utmost efforts to keep them out of manuscripts. A line in Lowell's *Fable for Critics* seems to reveal a psychological need for our inevitable lapses:

> One longs for a weed here and there, for variety.

And in *Idylls of the King*, Tennyson apparently decries perfection:

> He is all fault who hath no fault at all.

We look upon the Greek and Latin classics as the acme of perfection and as models for the molding of literary style and taste, but not all the ancients wrote supremely well. Numerous imperfections that one now finds in manuscripts, such as errors of fact, lapses of memory, faulty syntax, triteness, dud figures of speech, obscurity, verbiage, and pomposity, had counterparts in antiquity, so that a modern editor has to do with age-old problems.

Any critical person can cull from his general reading examples of errors of fact and lapses of memory. An informative article in a good magazine begins inauspiciously with the remark that *gastropod* is a word of Latin derivation. And the latest revision of a manual of botany lists numerous names of Greek origin under the heading "Index to Latin Names of Families, Genera and Species." In a recent issue of our most

literary magazine, a gifted scholar speaks of *Webster's New World Dictionary*, a glaring inadvertence that all readers of the manuscript and proof should have noticed. (The word *international* subconsciously suggested to the author the partial synonym *world*.) Another careless error was that of a columnist who, although surrounded with reference books, ascribed to Socrates (instead of to Protagoras) the saying that man is the measure of all things.

There are two errors that may rightly be called famous. In his sonnet "On First Looking into Chapman's Homer," Keats puts Cortez (instead of Balboa) on a peak in Darien to view the Pacific Ocean. Still better known is Dr. Samuel Johnson's mistake in defining "pastern" as "the knee of a horse."

To go back still farther, we find Greeks with faulty memories assigning to Homer familiar verses that occur in later writers.

We would not have so many errors like these to regret if we had all been endowed with the native scholarly caution of the cloakroom attendant in this ingenuous story:

> President Eliot of Harvard once was so delayed in reaching a lecture hall in New Orleans that he did not wait for a check for his hat as he hurried through the anteroom to deliver an address. At the end of the meeting, he was impressed by the unerring promptness with which the old negro in charge of the cloakroom returned his hat. "How did you know this was my hat?" he asked. "I didn't know it was yo' hat, suh, all I knew was that it was the hat you gave me." (*1*)

At present, however, I am primarily concerned, not with erroneous statements, but with matters of form and style and the more or less mechanical aspects of preparing manuscripts.

In a preface, a well-known author who both writes and speaks in a manner to command attention combines a generous acknowledgment of help received with a confession of his need for it:

> The best literary critic I have ever had has been my wife, and anyone who profits by this book is in her debt, and more especially for the absence of numberless words, phrases, sentences, and paragraphs that would have been in it had she not ruthlessly cut out the excess verbiage. (*2*)

Since experienced authors take precautions against having infelicities appear in their books, it seems strange that anyone should be sensitive to criticism of a manuscript and should prefer to wait for reviewers to point out faults in the completed book. Authors ask for technical help from colleagues and other experts in their fields, but they find it hard to get a competent critic of form and style and still harder to discover one who will take the time and the energy to do a thorough job of criticism. I became panic-stricken whenever a referee confidently declared that a manuscript was ready for the printer except for typographic directions.

One of the first articles that came to my editorial desk contained several hanging participles. Since the author was not immediately available, I made my own corrections. When he received his proof, he hurried to a dean and angrily complained that McCartney was trying to teach him English. The dean replied: "McCartney is a new broom and he is sweeping clean. I, too, use hanging participles." The world would have continued to revolve if the participles had gone uncorrected, but the errors were symptomatic. The author's overconfident English and his methods caused misunderstandings and involved him in rejoinders and polemics.

Authors who practice craftsmanship in conducting research projects have told me, tritely enough, that it is more important to have something to say than to say it well, a remark which implies that craftsmanship in reporting investigations is unessential, even if permissible. This is an aspect of relativity that I find hard to grasp. Good form is expected and needed in every calling, trade, and profession. The ballplayer who acquired a reputation for batting with "one foot in the bucket" could not have been proud of his stance, even if his batting average was high. Nor is "hatchet-and-saw carpenter" complimentary. The saying that "he chops a log in two three times" represents a woodsman's opinion of poor form, and it has a wider application. During my editorship, many persons sent to me copies of obscure and awkward sentences that had been written by experts in their fields. Workmanship does matter.

My freshman rhetoric specified clearness, force, and beauty as the essentials of a good style. In certain kinds of writing, one expects to find literary graces, but if reports of investigations are clear and tidy, they will be forceful and, in the eyes of an editor at least, very beautiful. The comparative rarity of such an accomplishment makes a manuscript outstanding and an editor ecstatic.

Long ago a Roman schoolmaster named Quintilian advised authors to lay aside completed manuscripts long enough for them to seem like the work of others. The advice still holds good, for an occasional author could not tell me the meaning of certain sentences he had written a few months before. One denied the paternity of a "senseless" correction, and I had to show him the directions for it in his own handwriting. He had a distinguished forerunner in Robert Browning. Everyone is familiar with Browning's answer on being asked to explain an obscure passage in *Sordello*: "When I wrote that, God and I knew what it meant, but now God alone knows."

In these days, the young scholar is in a particularly trying position. Owing to the financial pressure upon him as a graduate student, he must win a higher degree in the shortest time possible, and after he secures a position, promotion may depend on his showing further promise as an investigator. Editors experience special pleasure in helping him to get a start, but the repeated submission of complacent, hurriedly written articles throughout a lifetime is unfair. Editors are as hard-pressed as professors, and in the long run authors who have to rework unsatisfactory manu-

scripts for articles and books do not save time. The submission of a "quickie" is really a discourtesy, for it takes time and energy that rightly belong to other authors who are impatiently awaiting attention.

Probably the soaring costs of printing will contribute indirectly to the better preparation of manuscripts, since they are lessening an author's chance to have hasty work accepted. As in newspaperdom, some journals have been forced to cease publication; others have reduced the number of issues in a volume and also the number of pages in an issue. A few now use the offset process and do not submit proof to authors, which means that some blemishes in manuscripts will appear in print and that the proverbially best second thoughts will be lost to the world. Some editors and publishers reject articles that are long or contain matter expensive to set (tables and Greek, for instance) or else accept them only if they are subsidized. And more journals are being compelled to charge authors for corrections. These developments suggest that a larger number of poorly prepared manuscripts are going to continue to be just manuscripts.

To me it seems tragic that authors who are adding to the sum total of knowledge do not take time to discover or make use of the numerous aids available to them. The resources of the Merriam-Webster, for instance, are not generally recognized. Under the word *compound*, it gives the standard rules for hyphens, the discriminating use of which would make an immediate favorable impression on an editor or a critical reader. And the correct plurals of Greek and Latin words that a few biologists never master are crying for attention under the word *plural*. Answers to many other questions, such as the proper capitalization of words used in special senses and the agreement of the verb with *per cent* (now generally written *percent*), are also given.

A scholar who plans to spend a lifetime in research should own, and study, these two invaluable books: *A Manual of Style* (*3*) and *Words into Type* (*4*). The first represents long experience in dealing with the problems of writing and printing. The second presents the results of a thorough reconsideration of the problems of authors and editors. It gives much information not readily available elsewhere and is not dogmatic in its recommendations.

There are countless articles and manuals designed to aid authors, but, like the stream of articles on how to reduce, they get comparatively little result. Authors used to tell me that the aids I recommended to them afforded them no help for their particular ailments, but they proved indispensable to me in doctoring these same ailments and many others. I have listed on pages 133–135 of my book, *Recurrent Maladies in Scholarly Writing* (*5*), some of the more recent publications in the same field as my book. The articles by Bruner, McKerrow, Nicolson, Riker, and Silver are especially useful. The latest indictment of present-day writing is that by Jacques Barzun, "English as She's Not Taught" (*6*).

Authors should learn from the manuals the technical terms used by editors and printers and should spend a few hours in a large printing establishment to familiarize themselves with the various operations of printing and bookmaking. Those who cannot understand why a few corrections ("almost none at all") cost so much should give special attention to the method of making them.

Any author who expects to have extensive use for illustrations should pay an occasional visit to an engraving firm to learn both the potentialities and the limitations of the engraving processes. I repeatedly had to accept line drawings containing faults that a fuller knowledge of engraving requirements would have enabled the authors to avoid. Great care should be taken to guard against oversights in the drafting. It is costly, and sometimes impossible, to correct errors that are unnoticed until after the engravings (cuts) have been made.

It should be superfluous to implore authors once more to prepare articles in the style of the journal to which they intend to submit them, but constant reminders are necessary. If a journal does not have printed or typed directions, careful examination of a few issues will enable an author to determine the main features of its style. There must be close editing of articles in a periodical if the conflicting preferences and usages of the contributors are not to make its issues look unedited. If one may employ a stereotyped exaggeration, "it is impossible to overestimate the importance" of conforming to a journal's style. An editor's style sheet is his Magna Charta, but he often has to defend it against Declarations of Independence.

Observance (not just observation) of a few instructions would facilitate the work of editors, lessen expense, and win the enthusiastic cooperation of the printer's staff, an extremely important consideration that seldom occurs to authors. I know of no editor whose pathetic pleas or arts of persuasion have induced all the contributors to his journal to carry out directions like the following.

1) Write in clear, simple English. Long, involved sentences generally indicate that an author has not thoroughly assimilated his material. Sentences about which one feels uneasy are almost always faulty.

2) Guard against the unwitting and unremitting repetition of words and phrases. *Case, show, found, make,* and *use* are tiresomely overworked by some biologists and geologists. *Case* has occurred four times in 20 words; *show*, 12 times on one page; *found*, 57 times in a rather short article. Longer words like *apparently* (repeated 14 times in a brief paper) and *incidentally* become mannerisms.

3) Try to avoid the monotonous repetition of sentence structure and also a succession of either long or short sentences.

4) Insert all diacritical marks in quotations from foreign languages. Add them immediately after typing the letters that need them. If the keyboard does not have them, write them in with a pen.

5) Write "set as typed" or "OK as typed" above words that might be regarded as mistypings or that might otherwise cause misunderstanding. *Diplomate* has been

changed to *diplomat; empathize* to *emphasize; hemistich* to *hemstitch*; and *precession* (said of equinoxes) to *procession*. Write "OK as set" over such words when correctly set. An intentionally reversed C that was correct on all proof was noticed by a pressman at the last moment and changed to a normal C, much to the author's dismay.

6) Use opaque paper of good quality, not flimsy, transparent grades. Paper is the least expensive thing involved in printing. Leave ample margins. Manuscripts must be typed and be reasonably free of interlineations. Do not submit carbon copies, which are a source of both error and vexation.

7) Discard faded and tattered typewriter ribbons. They are an expensive economy.

8) Double-space everything, *everything*. EVERYTHING—quotations, footnotes, tables, lists in columns, captions, bibliography. The extra space will aid editors, keyboarders, proofreaders, and, eventually, yourself. The editor will give proper directions for reduced type.

9) Group footnotes at the end of the article. A quagmire of text and footnotes is a constant irritation to referees and editors who wish to follow the development of the thought. Copy for reduced type is set up separately, and some firms that are trying to keep costs down use the scissors-and-paste method of assembling it when it is scattered. Notes are distributed in the page makeup unless editors prefer to keep them at the end. Phrase footnotes as carefully as the text. Like shoes, they are noticed.

10) Do not crowd tables. They are expensive to set and difficult to correct. Authors show unpraiseworthy ingenuity in compressing on one page matter that should be double-spaced and spread over two or three pages, but they do not wish to pay for corrections. Cramped tables leave no room for an editor to correct inconsistencies, improve the form, or give directions to the printer. Periods are not used after incomplete sentences in tables. Since tables are set separately, no text should be put on manuscript pages that contain them. Excellent models for the handling of various kinds of tabular material appear in *Words into Type* (*4*).

11) Use mechanical lettering in line drawings and on maps. Lettering devices have greatly improved the appearance of such illustrative material in the last 25 years.

12) Do not be content with makeshift maps. Irrelevant or illegible printed matter on adapted or adopted maps disfigures articles and books and quickly destroys an editor's pride in his work.

13) Draft a friend to study, not just to read, your manuscript.

14) Check the (supposedly) completed article, especially quotations, references, and bibliography. These parts of manuscripts are generally unhappy hunting grounds for editors, since authors do not realize how readily mistakes insinuate themselves into them. In one set of manuscripts the 10-word title of an annual publication that I edited appeared with eight variations. Notes hastily made in libraries, especially those containing quotations and titles in foreign languages, are likely to cause trouble later on for both author and typist. Be very distrustful of bibliographic data taken from secondary sources. Make sure that you have supplied all the data required by your editor and that you have observed his sequence of items. Data for books not immediately available can usually be checked by Library of Congress cards or by reference books familiar to reference librarians.

The work of many persons is involved in the publication of a manuscript, and there are numerous chances for misunderstandings and mistakes. At some time or other, mortifying errors appear in the writings of almost everyone who publishes much. To lessen such risks, the initial stage, the manuscript, should be as nearly perfect as an author can possibly make it. Last-minute corrections sometimes cause exasperating fresh mistakes. Words are tools, and publications are like mechanisms in that they do not fully serve their purpose unless all the "bugs" have been removed from them.

Once in a while an author asserts that he, and he alone, is responsible for the content of his manuscript and the way he expresses himself, but the reputation of a publishing house or of a university press depends on the quality of its output, and it has both the right and the obligation to qualify its acceptance of a manuscript. And what editor is unconcerned about his professional standing? If the Constitution gives an author freedom of speech (and writing), it confers upon an editor the conflicting right to pursue (but not to overtake) happiness.

Even for his own selfish interests, an editor should do his utmost to please authors as well as the publisher, and each author should have the opportunity to pass upon all changes proposed. Editing is a matter of give and take. The problem of reaching an agreement is seldom difficult when an editor and an author can confer personally. It is somewhat harder when corrections have to be made by correspondence, because written criticisms seem harsher, for there is no facial expression or modulation of voice to show the kindliness one feels. No one relishes criticism, but acting the role of professional faultfinder is as distasteful to an editor as it is burdensome. No plaudits greet such acting.

Cordial cooperation elates an editor. On returning a paper to a Japanese author for revision, I expressed regret that I could not find any more ways to heckle him. In his reply he said: "I think you heckled my paper very well."

Bacon tells us that writing maketh an exact man. He does not specify the amount of writing required.

References and Notes

1. From *The Diplomate* **15**, 119 (April 1943).
2. The wife may not have had a chance to prune "excess verbiage."
3. Univ. of Chicago Press, Chicago, 11th ed., 1949.
4. Appleton-Century-Crofts, New York, 1948. The book is based on studies by Marjorie E. Skillin, R. M. Gay, and other authorities.
5. Univ. of Michigan Press, Ann Arbor, 1953.
6. *The Atlantic Monthly* **192**, 25 (Dec. 1953).

For 30 years, Dr. McCartney was editor of scholarly publications at the University of Michigan. This article was invited for Science *because his thoughtful and explicit suggestions for the preparation of manuscripts are applicable in any subject-matter or publishing field. Dr. McCartney's book* Recurrent Maladies in Scholarly Writing *is reviewed on page 544 of this issue.*

Conference on Scientific Editorial Problems

With the assistance and encouragement of R. L. Taylor, of the American Association for the Advancement of Science, and Donald H. Hale, Colonel, U.S. Army Chemical Corps, the first Conference on Scientific Editorial Problems was organized for the 1952 AAAS meeting in St. Louis. The purpose of the conference was to bring before the Association some of the important problems that confront those who prepare scientific manuscripts, who are concerned with the preparation of technical reports, or who edit and produce scientific publications.

The first conference was attended by 75 persons representing many phases of scientific writing, editing, and publishing. Speakers included A. J. Riker, "Standardization of literature citations"; J. Cattell, "Offset lithography"; Gertrude Mary Cox, "Role of statistics in technical reports"; L. E. Neville, "Problems of documentation in the Department of Defense"; G. Seielstad, "Format of technical reports"; A. E. Tyler, "Technical reporting in a Naval Research and Development establishment." At the 1952 session, it was decided to make the conference a permanent part of the AAAS meeting. The chairman appointed a steering committee consisting of the six speakers and Jonathan N. Leonard.

At the second conference in Boston, Dec. 27, 1953, attendance increased to 200 persons. Papers were presented by W. A. Noyes, Jr., M. O. Lee, G. S. Tulloch, Ruth C. Christman, R. M. Hewitt, J. D. Elder, R. B. Smith, and R. R. Shaw. (The papers are printed in this section.)

Multiple sessions are being planned for the 1954 conference to be held at the AAAS meeting in Berkeley this December. Members of the 1954 steering committee are: J. D. Elder, Harvard University Press; R. D. Hemens, University of Chicago Press; R. M. Hewitt, The Mayo Clinic; M. O. Lee, *American Journal of Physiology*; J. N. Leonard, *Time* Magazine; L. E. Neville, Armed Services Technical Information Agency; A. J. Riker, University of Wisconsin; G. Seielstad, The Johns Hopkins University; A. E. Tyler, U.S. Naval Ordnance Test Station.

The Conference on Scientific Editorial Problems invites the participation of all interested persons and groups. The 1954 sessions will include speakers on a wide variety of editorial subjects, and group discussions will also be scheduled. Programming is designed to examine questions and problems of interest to the greatest number of participants. Inquiries and suggestions will be welcomed. Correspondence should be directed to any member of the steering committee or to the conference chairman.

In these days of accelerated and expanded research by universities, industry, and government, and increasingly large numbers of scientific papers and technical reports, publication problems merit serious consideration. There is considerable need to standardize—certainly to discuss—techniques involved in effective technical writing and publishing.

MARIAN FINEMAN
Conference Chairman

Editorial Branch
Dugway Proving Ground
Dugway, Utah

Probable Trends in Scientific Publications as Viewed from the Editor's Office

W. Albert Noyes, Jr.

***Editor*, Journal of the American Chemical Society**
University of Rochester, Rochester, N.Y.

When one talks about trends, one talks about one's own journal; therefore, what I have to say may not be representative of science as a whole. The problems of the editor today are not entirely financial; they arise to some extent merely from the bulk of the things he has to handle.

The *Journal of the American Chemical Society* today publishes something over 5 times the number of words it published in 1920 and about 2.6 times the number of words it published in 1940. During the 4 brief years that I have been editor, the size of the journal has increased from 4100 pages to 6500 pages; we also print 10 percent more words per page. I can say only that, if this trend continues, a new editor will be needed before very long. The increase, incidentally, is not peculiar to our journal; the *Physical Review* and the *Journal of Chemical Physics* have increased almost exactly in the same ratio.

I have several thoughts about the kinds of manuscripts we receive today. In the first place, there is more fragmentary publication. I suppose that, as the number of chemists and scientists increases, the competitive spirit is bound to become of greater and greater importance. We find authors feeling that they must have rapid publication. They do not wish to wait for posthumous publication. Since presidents of universities base promotions not on the quality of the work but on the avoirdupois of the reprints, it behooves scientists to get out many small articles rather than one big one.

The editors also have been responsible to some extent for this trend, because, with the increase in receipts, it has been necessary to reduce articles to what some people facetiously call the telegraphic style. Unnecessary words have to be omitted; we have to eliminate graphs and tables as far as we possibly can. Consequently, one finds authors who say, "Frankly, I made this paper twice as long as I knew you would accept, because it gives me some leeway when you ask

me to cut it." This is the kind of thing that I as an editor deplore; this procedure wastes my time; it also wastes the author's time because he inevitably has to rewrite the manuscript. Frequently one feels that it would be better for the author to wait until he has enough to make a good manuscript rather than to submit something that is trivial, unimportant, and only part of a large picture.

There are various reasons for this large increase of routine articles since the war, not the least of which is the availability of government money in the form of university contracts. This kind of research is often of a type different from that in vogue before the war when a man shut himself up in the corner with a few graduate students and did what he chose to do. We now have large laboratories, where teams of people work together, where the crank is turned, and results appear without careful thought of their meaning and their interpretation. Moreover, some of these institutions now employ professional writers who do not know exactly what the scientific work is all about and who send us manuscripts that obviously place their emphasis where the author, himself, would not have intended it. A large amount of relatively routine material is submitted to us, and one begins to ask whether a journal that publishes 20,000 copies should accept material which has primarily only an archival value.

About a year ago, the board of directors of the American Chemical Society thought of trying an experiment, an undertaking that made us shudder. They said in effect, let us make the *Journal of the American Chemical Society* into a super journal. Let us take, for example, the best 20 percent of the 3600 odd manuscripts we receive each year and put them into the *Journal of the American Chemical Society*. Let us then establish subsidiary journals or sections of the big journal, if you wish, that will publish the remaining material which should be recorded somewhere but which is not very startlingly new. This made us shudder for several reasons. In the first place, it would have made the editors responsible for academic promotions. Another reason became apparent after we tried to look at the papers and to pick out the best 20 percent. Two assistant editors and I could agree easily that about 5 percent of the papers belonged in the super category; we could also agree that about 50 percent did not belong there; the remaining 45 percent proved extremely difficult to handle. We are grateful that the board of directors backtracked and decided not to make us carry out this program.

What is the quality of the scientific work? We tried to go over the manuscripts we received and to choose those in which there was an honest effort at interpretation of the data and correlation of facts—in other words, something more than the mere description of a routine synthesis or a routine set of physical measurements. Dr. Gates went over the organic and biological material; I went over the inorganic and the physical; our percentages checked exceedingly well. We both found that about 12 percent of the manuscripts really had some meat in the way of scientific discussion; the rest were mostly either recordings of routine observations, syntheses of compounds, or measurements.

I believe that in the near future we will have to decide whether a journal like the *Journal of the American Chemical Society*, which pretends to cover all of chemistry and which prints 20,000 copies of each issue, should accept manuscripts that have little reader value. Admittedly, all valid facts should be put somewhere in the records, because occasionally somebody will need them, but I think that we will have to confine our publication, with its large subscription list, to articles that have something more than just a few isolated facts. The solution, I think, is going to be some device such as microfilm or microprint, which is read with special glasses, or perhaps the formation of more specialized journals, leaving the big journals with large subscription lists for the articles that have some reader appeal. The chemist, of course, does not want to use a reading device. He frequently likes to sit in the laboratory and wave a bunsen burner at something while he reads a journal, and it is not convenient to have a viewer or a special pair of glasses on at the time. Hence, I do not think that the microcard idea will be universally accepted. In addition, the chemist, when it comes to his own field, is a most conservative gentleman. Hence, my prediction is more specialized journals; we will have to be more discriminating in the choice of articles we publish in journals of large circulation. This procedure will leave the problem up to the poor librarian who will have to get the funds to pay for the subscriptions to the specialized journals.

Problems in Financial Management of Scientific Journals

Milton O. Lee

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These remarks on the financial management of scientific journals are intended to apply only to the group of scientific journals that have the following common characteristics: (i) They perform important and/or essential services for communication within their fields. (ii) They are the official organs and, therefore, the responsibilities of scientific societies. (iii) Their circulations are small. (iv) They are devoted largely or wholly to the original publication of the results of research. (v) They are specialized in content, often highly so. The number of such journals in the United States is unknown, but it is large. The birth rate is higher than the mortality; hence, the number is increasing, particularly in newer, highly specialized fields of scientific research.

Each of the common characteristics listed in the preceding paragraph imposes more or less specific financial problems which, although varying in degree among journals, combine to present a rather uniform picture. There is a constant struggle to keep a journal solvent; its physical quality tends to be mediocre; and there is often a losing struggle to meet the pub-

lication demands of the field represented. The most common cure for these ills has been for dissatisfied groups to start new journals without much practical regard for their nurture and growth.

Scientific societies are formed and exist mainly for the purpose of communication in one form or another. Once formed, they usually become the responsible agencies for maintaining communication within their respective fields. Their jorurnals, as they become established and recognized, acquire a special significance in the furtherance of our scientific culture. Their independence, usually high standards for quality of scientific content, and responsibility to the best interests of their fields have often placed them in the enviable position of being almost indispensable.

However, most of our scientific societies operate on very modest funds derived from nominal membership dues. Membership in them is usually considered an honor, a recognition of professional attainment and competence. As with most honors, the recipient tends to consider himself on the receiving end only. Society members in general seem to be reluctant to accept the financial responsibility for support of their journals. If they are willing to require journal subscription as part of their dues, they set unrealistically low rates for themselves and place the rest of the burden of financing on nonmember subscriptions. These are increasingly from libraries, laboratories, and institutions rather than from individuals.

The expenses of scientific journals are conventionally broken down into the following costs: manufacture and distribution (composition of text and tables, imposition, presswork, binding, paper, covers, wrapping, addressing, mailing); engravings; printing, binding, and shipment of reprints; servicing of advertising; discounts to subscription agencies; storage and servicing of back issues; personnel services, editorial and business; and rent.

There are numerous and fairly obvious ways of keeping the cost of production of our scientific journals minimal or of decreasing it in many cases. These include the use of economical formats, page size and makeup, keeping to even signatures to fill forms, grouping and makeup of cuts, reduction of size of illustrations, simplification of tables, omission where possible of rules and boxed heads in tables, and in some cases the reproduction of tables as cuts.

The editors of journals can do considerably more than their printers in reducing costs. They can insist on reduction of the amount of verbiage in which many authors conceal their contributions; they can insist on the simplification of tables and on these being prepared to fit the format of the journal, on simplification of mathematical derivations, and on restriction of bibliographic citations to those specifically pertinent to each paper.

Many research papers are documented with detailed or extensive tables of data, illustrations, derivations of mathematical equations, protocols, appendices, detailed description of methods, long bibliographies or other materials that are of specific use and great value to a very few other workers in the field and that are expensive to compose and print. To the majority of readers of the published paper, however, the detailed documentation is neither necessary nor desirable. The relatively high cost of its publication in the printed journal is not justified, provided that the easy availability of the material to the few who need it is assured.

There fortunately exists a mechanism, through the Auxiliary Publication Service of the American Documentation Institute (administered by the Library of Congress), whereby editors of scientific journals may deposit such detailed documentation in lieu of printing it. Copies of material so deposited are available in photostat or microfilm form to any worker who needs it in the future; a footnote carried in the paper published in the journal calls attention to the deposition of such material and gives directions for obtaining it. This service should have much greater use by editors and authors than it has at present and could serve to reduce considerably the cost of publishing minutiae that are of high value to a relatively few readers. It involves, of course, the acceptance by both authors and readers of the desirability or necessity of restricting the amount of such minutiae in our printed journals.

On the business side, expense can be decreased by the elimination of discounts to subscription agencies, by insistence on payment of subscriptions in advance, by use of machine methods and standard business forms whenever practicable for billing, records, and accounting.

A journal derives earned income from the following sources: subscriptions; sale of single copies and back issues; sale of reprints; advertising; and, in some cases, authors' charges for space.

On the side of increasing the income of journals, the possibilities are usually greater than for decreasing costs. Subscription prices can be set at levels that are realistic in terms of actual costs of production, including editorial services in addition to manufacture and distribution. Many scientific journals now derive the bulk of their subscription income from libraries, laboratories, foundations and industry, rather than from individuals as subscribers. Even to those with a large individual subscribership, the income from these other sources is important. Subscription prices can, in many cases, be increased moderately over present rates without losing this support.

Heretical though the suggestion may be, there seems to be no valid reason for the members of a society paying a smaller subscription price than that paid by nonmember individuals or libraries. If membership entails the responsibility of helping to maintain communication within a field, does not a society member's responsibility include fully as much financial support to the society's journal as is asked from a nonmember? If such a policy were generally adopted, it might act to decrease the number of specialized societies in which a scientist holds membership, but it should serve to sharpen his interest in and responsibility to

those in which he wishes to retain membership. Concentration, rather than diffusion, of interest and support has its advantages.

The maintenance of stocks of back issues extending far into the past does not seem to be warranted in these times. Once they were necessary to insure that scientific publications would be available indefinitely to users anywhere. Acceptable substitute mechanisms are now in use to discharge this obligation—through microfilming, microcarding, and offset-reprinting. The cost of printing, storing and inventorying of large stocks in excess of the subscription list is considerable. The demand for back issues falls off sharply after from 2 to 5 years. It is questionable that the investment and servicing for stocks larger than needed for this short-time demand are warranted.

Reprints of published papers are often sold at practically the cost of the press work, paper, binding, and shipping. Realistically, their pricing should include a portion of the composition cost of the paper and of the editorial office overhead.

Many societies seem to feel either that their journal has something unique to offer as an advertising medium, or that industry as a gesture of good will should buy advertising space. This is an extremely naive point of view. The sales and promotion department of any industry is not interested in charity, and the mixing of charity and business is resented. In general, small-circulation journals can look for little support through advertising. In any event, they are not equipped to properly handle and service advertising contracts.

The picture as regards advertising is not wholly black, however. Certain specialized journals may be ideal media for the advertising promotion of some specialized items of industry. The problem of proper servicing of advertising contracts, usually far beyond the capability of the volunteer editor or business manager, still remains, but the possibility exists of a group of journals in a broad field—botany, zoology, physics—sharing cooperatively the services of professional advertising representatives who can do the work of servicing and distributing advertising in a group of journals in a thoroughly competent manner.

Some journals are now making a practice of charging a page rate for publication, or of charging for the cost of tables, illustrations, and formula material above a certain free allowance. This is based on the following premises: (i) communication through publication represents the final step in almost any nonclassified research; (ii) the research project has probably cost from several to many thousands of dollars; (iii) it is false economy to stint on publication—the final and essential stage of the research (actually 1 or 2 percent of the funds used for almost any research would cover the costs of publishing its results; (iv) it is essentially fair that the original publication of the results of research be, to some extent, at the expense of the funds that supported the research.

This practice is a rather unpopular mechanism at the present time for meeting financial problems of scientific journals. The experience of those groups that have employed it, however, has been that author acceptance can be achieved, that it provides a necessary relief of the burden on subscribers and therefore leads to wider use, and that it need not result in authors having to meet such charges personally. Before the practice is widely accepted it will be necessary for those who plan and prepare budgets for research projects to include an item for publication.

The desirability has frequently been stated for mergers of already existing journals, for the expansion of some to include subsidiary fields that are pressing for the initiation of new journals, and for the sectionalizing of large journals in order to avoid separate new ones. In many cases, considerable advantage would result, both economically and in effectiveness of science communication. It is obviously true that, in certain areas of science where journal development has proceeded along these lines, the whole matter of communication is more effectively handled than in other areas where unrestrained competition and division of effort has been the rule.

Our scientific journals, containing as they do, the permanent records of research, must certainly continue their existence, but they have the responsibility of discharging their function in an effective and economical manner.

Problems of the Editor of a Small Journal

George S. Tulloch

Editor, Bulletin of the Brooklyn Entomological Society
Brooklyn College, Brooklyn, N. Y.

The editor of a small journal usually has duties far beyond those of selecting and preparing articles for publication. Extra and related duties seem to rub off on him. He may find himself acting as subscription manager, advertising agent, head of the complaint department, and last, but not least, the custodian of all back issues of his journal. After a bit of history, I shall restrict my remarks to the problems that are of an editing nature.

The Brooklyn Entomological Society was founded in 1872 by a small group of men who were drawn together by a common interest in insects. Most of these men were born in Germany, where natural science was an important part of the curriculum, even of the elementary schools. During the early years, the meetings were conducted in German and were held in the backroom of a local beergarden. Within a few years, this group increased in size, and many native Americans were brought into the society. By 1879, the meetings were conducted mostly in English, and the meeting place was moved to the backroom of a store that specialized in the sale of insect specimens and entomological supplies.

In 1879, the society started publication of a monthly journal devoted to the topics of general entomological interest and to the insects of the area around New York. Although very few of the members were pro-

fessional entomologists, the journal was and still is devoted to papers of scientific value, thus precluding the inclusion of articles of popular interest. Owing to a variety of circumstances, publication of this journal was discontinued in 1885. It was reactivated in 1912 and has continued during the last 41 years. The 1953 volume is the 48th in the series.

Our society, through its monthly meetings, is dedicated to serve the people in our community who are interested in learning more about insects. Through our publications, the *Bulletin*, which is devoted to short articles, and *Entomologica Americana*, which is devoted to monographic works, we serve entomology as a whole. The subscribers to our *Bulletin*, which is my specific responsibility, are for the most part the educational, governmental, and research institutions, located all over the world, that include entomology in their sphere of interest.

For the most part, our *Bulletin* reaches the reader not at the retail or individual level but at the wholesale or institutional level, and this latter fact is reflected in our small subscriber list. We estimate that we reach about 2500 readers on a regular basis, which indeed is a very small constituency. However, based on bibliographic references to articles that have appeared in our *Bulletin*, we estimate that our occasional reader audience numbers into many thousands.

I am certain that the problems discussed here are not peculiar to editors of small journals, but I am not competent to judge which of my problems are characteristic of the larger journals. The following items appear to be the most important insofar as I am concerned.

1) *Maintaining reader appeal.* The problem of maintaining reader appeal is tremendously acute to editors of small technical publications. Our readers are limited to those whose interests are directed to the splinters of a subject rather than to the subject as a whole. The subject matter of entomology is enormous and, over the years, has been fractured into thousands of splinters. Those with taxonomic interests may restrict their endeavors to a single family of insects or even to a smaller category, such as a subfamily or tribe. The entomological histologist may restrict his interest to a single type of gland or to a single tissue. To these investigators, as well as to a large number of other specialists, the editor must provide a publication that is sufficiently rewarding to maintain reader interest and, at the same time, adhere to the editorial policy of the journal.

A mixture of articles of general interest combined with topics of a specialized and highly technical nature is a simple solution. Usually, however, the more specialized articles outnumber those of a nonspecialized nature, so that there is difficulty in maintaining a proper balance. Possibly, I should not admit that I solicit articles on general topics but this is the truth. At the moment, I have an author preparing a review on the respiratory enzymes of insects, a topic of great interest because of the new understanding of the control of insects by chemical means.

2) *Budgets and costs.* It is desirable to operate within the income of a publication. At the present, each of our yearly volumes runs about 140 pages spread over five issues, and the cost is $3.50. The 1929 volume had 382 pages and cost $2. During the past 6 years, our printing costs have increased about each 2 years, the period of a union contract in the publishing field.

In an attempt to adjust to these new conditions, we have had to reduce the size of our *Bulletin*. We have not, however, cut our *Bulletin* to fit our income; that is to say, we have adopted the unbusiness-like policy of operating at a loss, which for the last 6 years has averaged about 20 percent. I must hasten to explain that, as a side effort, we publish the *Glossary of Entomology*, and part of the profits from this venture are used to subsidize the *Bulletin* deficit.

In former days, we gave 25 free reprints to each author, and we assumed part or all of the costs of special reproductions as well as special composition. These gratuities now are a thing of the past, and there seems to be no hope that they will be reintroduced.

3) *Overabundance of manuscripts.* It is strange, perhaps, to complain of an overabundance of manuscripts, but at times this seemingly healthy situation is a problem. It is discouraging to an author, particularly a young one, to be told that his paper cannot be scheduled for publication until some vague date about 18 months away. As a general policy, I have, in the past, discouraged authors from leaving their papers with me if I could not foresee likely publication within a year. This seems to be a humane act but one that an editor may regret at some later date.

4) *Scarcity of manuscripts.* Having too few manuscripts is really a serious matter for an editor. A year ago I was turning manuscripts away. In October, I suffered the greatest manuscript depression in my 6 years as editor and wondered whether I would have enough material for the second next *Bulletin*. In December, there was an overabundance of manuscripts.

Entomologists, although of divergent interests, are a closely knit group, and word that a journal is loaded with manuscripts or is rejecting manuscripts because of an oversupply soon gets around, then the famine sets in. Because of these experiences, I am lifting my sights a little and plan to work on a 2-years ahead basis.

5) *Selection of manuscripts.* Most of our contributors are professional entomologists or other workers who have attained stature in the field and are recognized as competent authors and present no problem in selection. A second group of contributors is made up of younger workers who are introduced to us by older workers who indicate that they consider the manuscripts being forwarded as worthy of publication. These usually present no problems, although exceptions crop up occasionally.

A third group of contributors is made up of individuals who appear out of the blue. Their offerings

are examined very critically, usually by authorities who serve as anonymous advisers to the editor. Approximately 10 percent of these are rejected. There still is another type of contributor to whom I shall refer to as the "eccentric," the individual who attacks the motives or character of another investigator. Manuscripts from this source present no serious problem, since they are rejected as being inconsistent with our editorial policy, but they are a nuisance, because time must be devoted to processing them.

6) *Technical editing problems.* Some manuscripts received are perfectly typed, punctuated, and arranged in the style of our journal and require a minimum of routine attention (20 percent). The remaining 80 percent range from nearly satisfactory documents to those that require complete overhauling and in rare cases retyping. In one instance, I received a manuscript that contained 25 compositional inconsistencies. I asked the author for permission to mark the copy so that corrections could be made by him. This permission was received and I returned the manuscript; in a few days, I received the following letter:

> On receiving your letter with the corrected copy of my manuscript, I was filled with mixed emotions. In the first place, in the sixty papers I have published, I never had one more marked. At first I was tempted to call back the plate and publish it elsewhere. However, on reading through the manuscript my opinion changed. Now my sympathies are with you. However, the entire situation has been a little embarassing. My son is in the process of taking a typing course, so he offered to type the article from my notes. He thought I would proof read it and I understood that he had proof read the material. Neither of us did proof read it.
>
> I am having the article retyped and I hope it will not require further editing. Thank you for your patience.

Illustrations for Scientific Publications

Ruth C. Christman

Interscience Publishers, New York

The author who is faced with the problem of illustrating his scientific or technical writings has a dual responsibility: he must select his material carefully so that illustrations and text complement each other, and he must prepare his illustrations in a form acceptable for reproduction. The first part of the task is important to both author and publisher, since in these days of mounting costs each illustration must be evaluated for its importance to the text. The other responsibility, that of preparing acceptable illustrative copy, may sometimes be passed to the publisher, but for the purposes of this discussion it is assumed that the author must submit copy ready for reproduction.

Before selecting or preparing illustrations, the author of either a paper in a journal or a book should familiarize himself with the requirements of the publisher, so that knowledge of such details as page size and the kind of paper to be used may be helpful in considering reductions and the quality of reproduction that is desired.

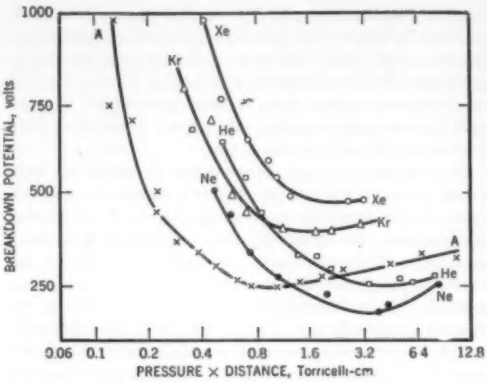

FIG. 1. A graph in which grid lines have been reduced to ticks to avoid confusion with curves. Symbols are well chosen and lettering is in proper proportion to the block. [Courtesy Interscience Publishers]

Of the two general groups of illustrations, halftones and line cuts, halftones are required when the material cannot be reproduced from lines but requires gradations of tone. Photographs form a large part of this group. Since halftones are more expensive than line cuts and require a better grade of paper in the printing process, the number of them is usually reduced to a minimum. Some authors are misled into believing that halftones that are acceptable photographically should be used to enhance the paper or book, even though they add little to the value of the text.

The value of a photograph to an understanding of the text should be considered carefully. Usually photographs are larger than the reproduction, and important details may be lost in reduction. Frequently illustrations of instruments or machines are so reduced in reproduction that the reader would be better served by a schematic or a flow diagram. When the required reproduction will result in a meaningless figure, it is sometimes possible to crop—that is, cut superfluous bordering portions from the photograph—so that the remaining featured part may be reduced less drastically.

If photomicrographs or electron micrographs are to be used, reduction problems must be considered carefully. Reduction in size may decrease magnification to a point that makes the illustration valueless. In addition, a finer screen and paper of better quality may be needed to produce satisfactory results. For these special problems, the advice of the publisher should be sought.

Halftone material submitted for reproduction should be handled carefully and be adequately protected against damage. Bending or cracking the surface will mar the reproduction. Pressure marks from pins,

staples, or clips are equally destructive. No marks of any kind should be made on the face of the photograph and, if for any reason, the illustration needs special attention, an overlay should be used on which to write instructions. The writing must be done lightly, because pencil-pressure marks will be reproduced. Care should be exercised even in writing on the reverse of the photograph, because pressure marks from the back may also cause damage.

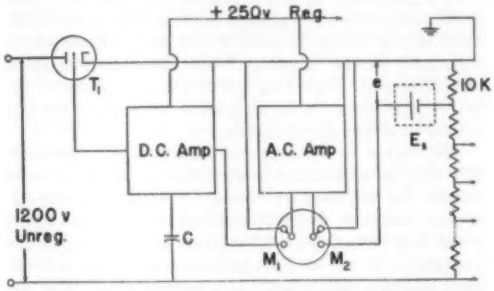

FIG. 2. A well-drawn electrical diagram shows the circuit uncluttered by details. [From *Astronomical Photoelectric Photometry*, published by AAAS, 1953]

Four kinds of line cuts are commonly used in scientific and technical writings: curves, block diagrams, bar diagrams, and schematics. To all, the same general principles of good draftsmanship apply. First, the simple instructions that are always given scarcely seem to need to be repeated; yet they are frequently overlooked or disregarded. Drawings should be submitted on Bristol board, tracing paper, or tracing cloth and should be drawn in India ink. It is advisable to make line drawings larger than the desired result, so that minor defects and unevenness will be reduced and, hence, will be less apparent in the reproduced illustration. Lines should be smooth and of the proper thickness for satisfactory reproduction. The choice of a pen is, therefore, of prime importance.

In planning a drawing, the over-all size must be considered in relation to the components. For example, a well-drawn graph may be unusable after reduction because the size of the lettering after reduction was not computed or visualized. Instructions are sometimes given to the effect that the reduction of the drawing must not result in letters less than a certain height, say 1/16 in. Height alone, however, is not the criterion of readability—letters should be well spaced, not too narrow, and should be drawn with the correct thickness of line. Too thin a line will break in reproduction, and a thick line will close openings such as those in "g" and "e". Hand lettering should be avoided, for even the best cannot give the evenness of line and uniformity of design found in mechanical lettering devices such as Wrico and Leroy. Stamping, when uniformity in the type for the text and illustrations is desirable, and gummed letters and numbers, when needed in limited amount, are also in general use.

All drawings should be planned to highlight and emphasize the text. To achieve this, heterogeneous bits of information should be eliminated, and the introduction of too many facts should be guarded against. Legend material should not be incorporated in the drawing but should be relegated to either the text or the caption. Abbreviations should agree with those used in the text to avoid confusing the reader and should conform to the conventions of the science or the particular publication in which the drawings are to be used.

Symbols on drawings should be well chosen for reproduction value. Open circles, filled circles, squares, and triangles reproduce well, if of sufficient size. If they are not sufficiently large, the openings tend to fill in reproduction. Half-filled symbols, for example, circles with the upper halves black and the lower halves white, are to be avoided, particularly when they are used with filled circles, since they will not be easily distinguished after reduction.

In preparing curves for reproduction, it must be kept in mind that fine blue lines will disappear in the photographic process. All lines, therefore, that are needed for an understanding of the graph must be inked in black. Grids in other colors should never be submitted for reproduction.

Grids should be carefully planned so that in reduction the grid size will be uniform within related sections of the discussion. For example, two curves that are reproduced for purposes of comparison should not

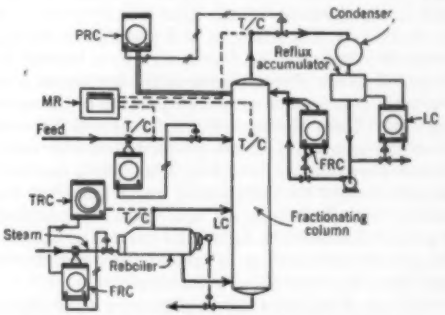

FIG. 3. A flow diagram of a control system for a continuous fractionating column. The principal parts of the system and their relation to one another are in a small, well-proportioned outline. [Courtesy Interscience Publishers]

be drawn on grids of different size. If the grids of one drawing to be reduced one-quarter are 1-in. square, another comparable drawing with grids 1½-in. square should be reduced one-half. Lettering, of course, must aso be carefully checked for likeness of size after reduction.

Too numerous grids, too close together clutter the drawing. If the curve is not to be used for computing numerous precise points, ticks may be used to indicate the scale. In other cases, lines may be more widely

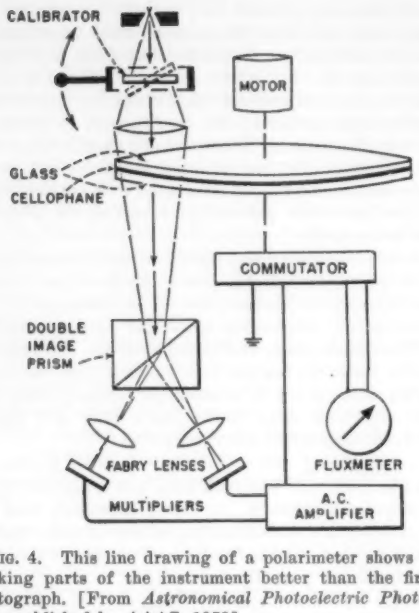

FIG. 4. This line drawing of a polarimeter shows the working parts of the instrument better than the finest photograph. [From *Astronomical Photoelectric Photometry*, published by AAAS, 1953]

spaced without jeopardizing the value of the drawing. When neither device may be resorted to, variation in the thickness of the lines will serve as an aid to readability. For example, if 5-point units are used, every 20th unit may be represented by a heavier line. Scale values are always shown on the horizontal plane, but scale descriptions should be parallel to the enclosing line. Theoretically, all graphs should be scaled from the zero point. Some scientists consider scaling from higher points a breach of form, but a reasonable attitude toward the inclusion of waste space has made scaling from higher points acceptable. For example, if a curve fills only the right-hand portion of the block, the points unrelated to it are cut from the drawing and, thus, the over-all size is reduced.

The use of bar drawings is sometimes more effective than tabular arrangement, especially if it is desirable to show the relation of two items under varying conditions. Block diagrams are also an effective means of conveying an idea, for example, to serve as flow sheets.

Schematic drawings are widely used and are especially valuable to show the working of a piece of machinery or an electric device. For these purposes, the details should be carefully chosen. When the scheme is simplified and the drawing is not cluttered with too numerous details, the effectiveness of the illustration will be increased. Changing the thickness of the line is also a means of emphasizing the important parts of the machine and increasing an understanding of its operation.

In all illustrations, we return to the important admonition to direct them to a single idea and, in line drawings, to keep that idea uncluttered by superfluous bits of information.

Jargon—Good and Bad

Joseph D. Elder

Harvard University Press, Cambridge, Massachusetts

It is recorded of Lord Rutherford that he "took great pains over his writings, holding that no scientific discovery is complete until it has been expressed in clear and concise language." The obligation to make oneself clear in reporting the results of a piece of research is as great as the obligation to be objective and honest in doing the research.

Although a writer may find clarity hard to achieve, difficulty in expressing one's meaning is not a valid reason for slipshod or muddy writing. An experimental scientist will not hesitate to take whatever pains may be necessary to improve the working of his apparatus, so far as his equipment or funds or technical abilities will allow. A voltmeter must give accurate readings, a meter bar must be of the standard length, a clock must run with a known rate, if the measurements made with these instruments are to be reliable.

In many laboratories, even the superficial appearance of the apparatus is given some thought. An amplifier does not work better because it is housed in a case with a black crackle finish, or because the knobs are arranged in straight rows instead of at random over the face of the panel, but it looks better that way. The neat appearance suggests that the maker is competent, and the user may even find the amplifier easier to operate. The same is true of a piece of writing. It ought to have the best finish that the writer can provide, so that the reader can use it easily and will feel that the maker is competent.

Moreover, as Rutherford held, the research is not complete until the results have been reported. What is the use of research conducted with the best apparatus, at great expense of time and effort and money, if the results of it are not communicated to all who might benefit from knowing them? Publication is the end-product of research. Research without publication is sterile.

How, then, can this last essential of scientific research—the preparation of a report, or the collection of the fruits of a long research program in a book—be best carried out? How can a writer express "in clear and concise language" what he has found out and the conclusions that he has arrived at?

One way in which he can improve the clarity of his expression is to avoid the use of jargon; another way, and one that may improve not only his clarity but his conciseness, is to use jargon well. Let us first agree on the meaning of the word *jargon*. The dictionary defines it as "confused, unintelligible language; gibberish; hence . . . the technical or secret vocabulary of a science, art, trade, sect, profession, or other special

group." For our purposes, a few words of this definition will suffice. No scientific writer will admit that he uses confused, unintelligible language, or that he writes gibberish, or even that his vocabulary is secret (no matter what others may say of it). But if the technical vocabulary of a science is jargon, then all writers of or on science use jargon. They must use it, for in no other way can they achieve the clarity and conciseness of expression that Rutherford called for.

Writers in any field of science are in a very favorable position in comparison with writers in, say, law or international affairs. They have the great advantage of possessing a jargon that has been deliberately constructed to serve their needs. Technical terms have been adopted and defined with the express purpose of giving all writers in the field words that they can use with the assurance that every reader, merely by looking at the definition, can tell exactly what the writer meant. Thus, a single term—a word or a phrase—can be made to stand for a whole paragraph of description. The word *power* in physics, whatever its meaning may be in the ordinary parlance, means "rate of doing work" and nothing else. A second of time is 1/86,400 of a mean solar day, and that is all it is. It is not necessary in scientific writing to use in a single statement all the words that may be construed to have some possible shade of meaning in common, as it seems to be in framing laws or insurance policies. No court is needed to decide what a scientist meant by what he said; he has already said it in the only possible way, and all who know the definitions of the terms he used can understand what he meant.

This is the advantage of jargon properly used. But one man's technical vocabulary is secret to another. Whether or not to use jargon in a particular piece of writing depends on the audience for whom the writing is intended. A physicist writing for other physicists may use the word *neutrino*, or *linear accelerator*, or *synchrocyclotron* without fear of being misunderstood; but if he is writing an article for a popular magazine, he must remember that for most of his readers such words do not have the precision of meaning that he himself gives them. Jargon is good when the reader can reasonably be assumed to know what it means, and bad when he cannot. Even when he does know the meaning of jargon, however, simple words can often be substituted for technical ones. The writer who is so enamored of his jargon, or who has so limited a vocabulary, that he piles up technical terms when simpler ones would express the meaning just as well does his reader a disservice, whether the reader is familiar with the jargon or not.

Part of the tendency toward the abuse of jargon begins when a graduate student prepares for his first colloquium talk. He knows that his audience will contain specialists, both professors and students, in his own field, and others of high competence in related fields. He fears that he will seem to be talking down to them, or not to have command of his subject, if he uses simple terms; so he chooses long ones. This habit, once established, is hard to break, and it leads to the misuse of jargon.

Here is an example of jargon well used: "The frequency separation between the diametral frequencies of the admittance and impedance diagrams affords a useful measure of the coefficient of electromechanical coupling." The vocabulary of this sentence is certainly technical, which makes it jargon, but for those who have the same vocabulary it is both clear and concise. The phrase "frequency separation" could be improved; "separation" does not really mean "distance," even metaphorically. It would be better to say: "The difference between the diametral frequencies. . . ." This use of the word *difference* assigns to it the meaning that it has in arithmetic; this is jargon that the readers to whom the manuscript is directed can be assumed to understand.

Each of the sciences has acquired its own jargon, sometimes by deliberate invention of a new word, but more often by an almost unconscious process of growth and development. There have not been many van Helmonts, to invent such a word as *gas*. But when *electron*, the Greek word for amber, was made to stand for the elementary electric charge, it set the pattern for a multitude of words ending in *-tron—neutron, positron, negatron* (now discarded)—for the names of subatomic particles. It may also have had something to do with the sound of names for pieces of apparatus such as *thyratron, cyclotron, betatron*. The word *proton*, without the *r* in *electron*, was doubtless the type for *meson*, which seems to be replacing *mesotron*. Since the two types of meson were distinguished by the Greek letters π and μ, the names *pion* and *muon* have come into being. (We may hope that the particles now called simply *V-particles* will never come to be known as *veeons*.)

These words were coined because there was need for a single, unambiguous name for each of the particles as it was discovered, a name that would be short, at least partially descriptive, and incapable of being confused with any other name. They are part of the jargon of physics and they are very useful—to physicists talking to other physicists. They are entirely out of place, however, in a popular article unless they can be defined in simple terms. Too frequently they are introduced into popular writing in a way that implies that the writer knows what they mean but does not think that the reader can really understand. The science writer for a newspaper who tells his reader that "the new particle, called a flyon, is one of the building blocks of nature" gives his reader no help at all by his use of a technical term. On the contrary, the reader cannot escape the feeling that the writer is showing off. It is such misuse of technical terms that makes *jargon* often a word of contempt.

It is not necessary to multiply examples of bad jargon, for they are all too common and well known. The careless or downright wrong use of the words of a scientific jargon is a consequence of the disregard, or even ignorance, of the properties and characteristics of the English language. One of the sufferers both from jargon ill-used and from slipshod language is the reader who may be expert in a science but to whom English is not his mother tongue. The intelligent for-

eigner will be able to understand this sentence from a medical paper on blood grouping, "The grouping sera may be prepared by immunizing rabbits and drying and using them in the powdered form," but why should he have to stop and straighten out such a statement?

Editors of scientific writing, whether technical reports, research papers, books, or popular articles, have obligations to both author and reader. For the sake of the reader, the editor must help the author to say what he means in the simplest way; to eschew jargon when it is merely high sounding, for then it is bad jargon; to use technical terms in technical writing when they lead to clearness and conciseness, for this is good jargon; to recognize the advantages of acquiring "the habit of paying all words the compliment of respecting their peculiarities." Good writing comes hard, but the gain is worth the labor. There is sound advice in the words of Isaac Watts:

> Smooth be your style, and plain and natural,
> To strike the sons of Wapping or Whitehall.
> While others think this easy to attain,
> Let them but try, and with their utmost pain
> They 'll sweat and strive to imitate in vain.

Publishing as Applied Science

Ralph B. Smith
McGraw-Hill Publishing Company, New York

The advancement of science depends upon publications that keep scientists in communication with one another. The advancement of civilization depends upon publications that keep the rest of mankind in communication with scientists.

If such verdicts tempt you to a good yawn, can I stop it with an accusation: That our publications often overlook their own dependence upon some of the science that they communicate. I feel that I am in a particularly good spot to see that publishing must apply science to its own job and that this application must go far beyond the mechanical and chemical processes which put that job into print.

I work for 27 publications that are read principally in the United States and eight publications that are read only in foreign countries, four of them in foreign languages. Dealing wholly with the world's work, not one of them offers a reader escape from his work. They provide entertainment only if they lapse into entertaining errors. No professional society lassos an audience for the words of any one of them—not a sinful practice, but not a practice at all in our case. Not one of them enjoys a franchise to channel to its reader group through its pages all the wisdom of the research or convention papers that emanate from the wise men of that group. Most decisive, not one of these publications may put into its audience a single customer who has not paid to come in and who cannot go out if he does not like the show. This is true for the whole line—from *Business Week* with its 250,000 readers to *Electrical Wholesaling* with its 9000—from the "horizontal" papers, such as *Electronics* or *Product Engineering*, to the "verticals," such as *Coal Age* or *Textile World*.

This is free enterprise with a vengeance. It is free of any protection against bankruptcy if its editors fail to ascertain what selection of content and what choice of presentation techniques will bring into their show enough of the people and only the people who should be there. It is no enterprise in which to play hunches, gamble on sheer editorial intuition, trust to tradition, or bet that habits do not change.

Nor is any business, industrial, professional, engineering, or science publishing enterprise whose journals are not simply given away to people on its advertisers' prospect lists. The cold facts that, perhaps, hit us first in our exposed position must also blow upon the somewhat less naked society and institutional publications. Indeed, I imagine that an ill-wind blowing upon them from some quarter of reader-discontent can be noisier, if not more disturbing, than the quiet dropping of renewal percentages that has been our historic warning of trouble. I am told that hell hath no fury like a dues-paying professional society member grown scornful of his society publication.

But complaints and cancellations come too late, and their absence is no proof that we are being read today—much less that we shall be read 5 years from today.

We are all meeting new competition for time—from new channels of communication and from new distractions. Our news trickles through a rising flood of information from other sources. Our readers are harassed by an acceleration of the tempo of living and of the rate of change. This is an unconventional world in which to do the conventional publishing job, and the whole situation seems to be forcing upon us—despite our present unprecedented success—the question of how much we know *scientifically* about *what* our readers now want of us and *how* they want it.

We at McGraw-Hill believe that we still have a lot to learn and a lot that we can learn by applying scientific research to reading. For a long time, we have been sending out Research Department interviewers to ask scientifically selected samples of our "populations" what they have read and what they have skipped in particular issues of our magazines; then calculating how all the pages of these issues stack up in relative headline-readership, starting-readership, and complete readership; then collating our information to determine how results for every page were influenced by type of content, by technique of presentation, and by classification of reader. We are now proceeding to code our accumulation of such data on punch-cards under a great variety of categories to get better and faster answers.

Our individual magazines are—and long have been—supplementing such reader-traffic field-studies by somewhat less valid, but still suggestive, mail surveys that poll readers on the relative interest of their various articles and departments.

We are also engaged in split-run experiments. This rather new research technique involves splitting the

press run of an issue of a publication to present the same editorial material in one way to one group of subscribers, in another way to a comparable group. Then we send out our Research Department interviewers to collect information on the readership of the issues on each side of the split. They tell us whether one version of the varied material gets more attention than the other. By varying the presentation of as many as six articles or departments in a single issue and by supplementing field interviews by mail surveys and by preference studies, in which we ask readers to choose between the two versions of the same material reprinted side-by-side, we check this experimentation from every angle.

We also devise specialized types of research to serve narrower purposes: to test our magazines against their competitors; to explore the potentialities of a projected new publication, or the value of expanding an existing one into a new field or of splitting an existing magazine; for example, *Product Engineering* was born for design engineers from *American Machinist*, our metalworking publication.

I have had to draw on my own experience for illustrations in support of a plea for the application of science to publishing. It would take more space than is available to record the pioneer work along the same lines for which some other publishers—notably in the general magazine field—should be given the credit. Reciting the full history of our own false starts and trials and errors, which leaves me with a feeling that we are only just beginning to make research pay off, would also take too much space, but the job that can be done is ahead of all of us.

If it is true that the eye-camera, the one-way mirror, split-run experiments, the use of specialized techniques to determine reader-preferences, reader-traffic surveying, and informed application of functional logic to editorial presentation are establishing scientifically verifiable facts about how readers react to content, how articles should be structured, illustration handled, and display editing used to the best advantage, what does this mean to the editor?

It means that editing is acquiring that body of learning which marks a true profession. It means that, however much our sources of information or our contributors may know about *their* subject matter which *we* edit, we stand to know much more than they do about when, where, and above all how it should be presented to the reader. It means that, if we can establish our right to their respect for this superior professional knowledge, we must demand that respect or sacrifice our integrity.

Conversely, it means that, in the face of the evidence, the expert in any subject matter with which we deal in our publications need not feel that he is sacrificing some integrity by an admission that he can be even drastically rewritten, or have his illustrations reworked, or his approach changed, or his headings reworded. It is my hope that science, in our hands, can bring him willingly, even eagerly, to such a conclusion, to the greater service of greater numbers of readers who need his help and ours.

News and Notes

Science News

Organizations throughout the nation are celebrating the **100th anniversary of the founding of the profession of entomology in the United States.** In 1854, the first two entomologists ever employed in this country were appointed, one by the Federal Government and one by New York State: Towend Glover was assigned to the U.S. Patent Office where he studied insects that attack orange trees and cotton; and Asa Fitch, employed by New York State, focused his attention on its local insect problems. From these small beginnings, the profession has grown until there are now 4500 men and women engaged in the science of insect control in the United States.

Atomic particles passing near to an **atom's nucleus** set up undulations over the nucleus surface; similarly, the moon regulates tides on the earth. Clyde McClelland and Hans Mark, working under Clark Goodman, associate professor of physics at Massachusetts Institute of Technology, have discovered that the waves on the nucleus surface take the shape of bulges that travel around the nucleus at definite speeds. The research group has found that tungsten nuclei absorb little energy when accelerated through 1 megavolt in an electrostatic generator; the energy absorbed appeared as pure rotational energy. The investigation is being sponsored by the Office of Naval Research and the Atomic Energy Commission.

High manufacturing costs in the publishing field make it increasingly difficult to place manuscripts likely to have a limited sale. Big commercial houses are more and more unwilling to risk financial loss and university presses, which were once the recourse of the scholar, find it necessary to ask for large subventions and guarantees. The **International Scholars Forum,** sponsors of a series of books by American scholars, has been organized in the belief that many first-rate manuscripts are circulating in vain search for a publisher. Publishing costs are much lower on the European continent than in this country; but unfortunately, American authors do not know much about European publishers, and these publishers, for their part, find it difficult to evaluate the American manuscripts submitted to them. The Advisory Board of the International Scholars Forum has therefore entered into an agreement with Martinus Nijhoff of

The Hague to receive manuscripts, appraise them, and make recommendations regarding publication.

An American publisher requires an initial edition of 2000 or more copies, whereas Mr. Nijhoff is prepared to publish an edition of as few as 500 copies. Authors who, on a realistic assessment of their sales potential, conclude that it will be limited chiefly to libraries and to other scholars in the field, should therefore consider the possibility of submitting their manuscripts for publication in the Forum. For further information apply to the Librarian of the Honnold Library, Claremont, Calif.

The following comments on the Atomic Energy Commission's suspension of **J. Robert Oppenheimer** appear in a news story prepared by Watson Davis, director of Science Service:

> The tragicomedy being enacted justifies the fears that many atomic scientists had in 1946. With the war emergency over presumably, they rushed back to the unsupervised and unrestricted quiet of colleges and laboratories. Some cut loose completely. Others returned to the AEC's atomic research only when the H-bomb major push began in 1950, but with reluctance and a high sense of national duty. The scientists did not relish the heavy and sometimes quite unintelligent hand of the military. They sensed the danger of liberals being red-baited.
>
> Dr. Oppenheimer could have had, but did not wish, major responsibility in the continuing atomic energy program after the war. He did serve in advisory capacities, from a sense of public service. The essentially political attacks being made upon him now are sorry thanks for his service to the nation.
>
> The attack upon Oppenheimer will not make the staffing of the atomic energy program any easier. If the atomic program for defense and industry is to continue successfully, there must be continuous research of the sort that Oppenheimer did. The innovators and the pioneers will not desire the risk of personal attack upon the basis of unevaluated FBI files.

Scientists in the News

J. W. Buchta of the University of Minnesota has been appointed executive secretary of the Advisory Committee on Government-University Relationships, at the National Science Foundation. Prof. Buchta will be on leave from his post in Minnesota as professor of physics and associate dean of the College of Science, Literature, and the Arts.

Bernard D. Davis, who is in charge of the U.S. Public Health Service Tuberculosis Research Laboratory located in the Department of Public Health at Cornell University Medical College, has been appointed professor and chairman of the Department of Pharmacology at the College of Medicine of New York University—Bellevue Medical Center. The appointment will become effective on July 1 when the present chairman of the department, **Severo Ochoa,** assumes chairmanship of the Department of Chemistry.

John C. Eberhart, chief of the Research Grants and Fellowships Branch of the National Institute of Mental Health, has resigned to accept appointment as executive associate with The Commonwealth Fund in New York City. **Philip Sapir** has been appointed acting chief of the Branch.

Joseph T. Flakne, recently appointed director of programming of The Arctic Institute of North America, has been selected to coordinate research activities in the Arctic Research Laboratory at Point Barrow, Alaska—the northernmost U.S. settlement in North America. As a result of negotiations with the Office of Naval Research, the Arctic Institute has assumed responsibility for the conduct of the scientific program in Point Barrow. Basic research relating to problems affecting northern engineering, communication, public health, and other fields will be carried on under the guidance of an Institute committee composed of 11 experts. It is expected that special emphasis will be placed within the province of the physical sciences, such as permafrost studies; investigations in the oceanographic field, including tidal movement and ocean currents; and hydrobiological studies. The Arctic Research Laboratory is the only station in U.S territory where fundamental research relating to the northern environment can be conducted on a continuous basis in a variety of sciences.

George Gamow, who is on leave from George Washington University, has been appointed visiting professor of physics at the University of California, Berkeley. During the current semester he is teaching two graduate courses, one on "Relativity and cosmology" and the other on the "Evolution of the stars."

Isidor Greenwald, who retired from the New York University medical faculty on Aug. 31, 1952 after more than 20 yr of service, has been appointed professor emeritus of chemistry of the College of Medicine of the N.Y.U.—Bellevue Medical Center. He is known for his challenge of the hypothesis that endemic goiter is due to a lack of iodine in the diet in any given geographic area. Since his retirement Dr. Greenwald has continued with his research on goiter.

Lauren B. Hitchcock, New York chemical engineering consultant, has been appointed president and managing director of the Southern California Air Pollution Foundation, Los Angeles. Eighty industrial and business executives organized the independent, nonprofit Foundation last fall to support and foster research or other means to solve the smog problem. The Foundation will function through a small group of specialists who will formulate the problems involved and encourage research projects.

The American Institute of Nutrition made the following awards at its annual dinner on Apr. 14:

The Borden Award in Nutrition of $1000 and a gold medal was presented jointly to **Agnes F. Morgan** of the

University of California, Berkeley, and **Arthur H. Smith** of Wayne University, for their important investigations on the effect of heat on the nutritive value of milk proteins and for their many other contributions during the past 30 yr concerning the nutritive significance of other components of milk and milk products.

The $1000 Osborne and Mendel Award was given to **Leonard A. Maynard** of Cornell University for his fundamental investigations on biochemical and nutritional aspects of lipid metabolism and of lactation and for his many contributions as a teacher, administrator, and public servant in the field of nutrition.

In March, **A. G. Newhall** of the Department of Plant Pathology, Cornell University, arrived at the Agricultural College of the University of the Philippines, Los Banos, where he will spend a year teaching and conducting research. He has replaced **G. C. Kent**, who has been in Los Banos for the past 18 mo. Dr. Kent will resume his duties as head of Cornell's Department of Plant Pathology in June after visiting several European universities during his return trip. Dr. Newhall and Dr. Kent are participating in a cooperative project of the Agricultural College of the University of the Philippines and Cornell University sponsored by the U. S. Foreign Operations Administration. The purpose of the undertaking is to assist in the rehabilitation of the Agricultural College.

Ralph G. Pearson, associate professor of chemistry at Northwestern University, and **I. M. Kolthoff** of the Department of Chemistry, University of Minnesota, are being sent to England by the National Science Foundation to participate in the Faraday Society discussions on "Rapid reactions."

At the Sixth International Congress on Leprosy in Madrid last fall, **H. W. Wade**, associate medical director of the Leonard Wood Memorial (American Leprosy Foundation) was elected to the Academica Nacional de Medicina and was also elected president of the International Leprosy Association for the third time.

Education

Although approved medical schools are now accepting their largest freshman classes—totaling almost 7500 students—the number of **applicants for admission to medical schools** has decreased for the fourth consecutive year, according to an article by John M. Stalnaker in the April issue of *The Journal for Medical Education*. The freshman class of 1953–54 had some 2085 fewer applicants than the previous year's class, and almost 10,000 fewer individuals are making application now than did in 1949–50 when the GI bill was in full force.

As Mr. Stalnaker points out, however, there are still more individuals seeking admission to medical schools than can be accepted, but many of them are not qualified for the long hard grind of medical school. The average applicant applies to several schools. Figures show that 23 percent of this year's applicants had sought admission to medical school the year before and were repeating, while the comparable figures for the preceding year was 31 percent. Thus not only are there fewer students applying, but fewer students are willing to continue to apply after having once failed to gain an acceptance. Forty percent of the reapplicants are accepted, compared with 57 percent of the first-timers. Averages on the Medical College Admission Test were slightly lower for the group applying for the second time.

Mr. Stalnaker noted that some schools had a wealth of good applicants; the competition for them is heavy, for such students usually applied to several schools and all are anxious to get them. The medical schools that limit their applications to state residents in many instances had to scrape the bottom of the barrel to secure a freshman class. Of the states supplying 100 or more applicants to medical school, New York had the lowest proportion of acceptances and Iowa the highest.

The Department of Psychology, University of Chicago, announces two 5-day **workshop seminars in the Rorschach Test**, July 6–10 and July 12–16. They will be conducted by S. J Beck. Workshop I, *Basic processes*, will provide a grounding in fundamentals. The procedure in obtaining the test record will be discussed. Representative responses will be illustrated and their scoring clarified, with especial reference to their interrelations in shaping the whole personality structure.

Workshop II, *Advanced clinical interpretation*, will consider the ego, anxiety, and the individual's psychological reserves as treatment potential. The cases will illustrate "schizogenic" conditions in children, and some milder disorders in children and adults; they will exemplify anxiety of "central" (inner) source, as well as of peripheral stimulation. In exploring for reserves, both structure and content will be scrutinized.

Workshop I may be attended by students at, or ready for, the interne level. Admission to Workshop II is limited to psychologists and psychiatrists in clinical positions or practice. Each seminar will consist of two sessions, each day, 2 hr per session. For information write to the Executive Secretary, Department of Psychology, University of Chicago, Chicago 37, Ill.

Grants and Fellowships

The following **AAAS research grants** have been awarded:

American Academy of Arts and Sciences to R. R. Gates, Peabody Museum, Harvard University. Race crossing in man and mutation and frequency of crossing in Oenothera.

Indiana Academy of Science to J. E. Potzger, Dept. of Botany, Butler University. Pollen research in Quebec.

Tennessee Academy of Science to Frank H. Barclay, East Tennessee State College. The vegetation of Johnson County, Tenn., with special reference to the bogs.

Tennessee Academy of Science to Richard Stevenson, East

Tennessee State College. Altitudinal distribution of Drosophila on Unaki Mountain, Tennessee-North Carolina, with special reference to an anomalous sex ratio in D. Affinis.

Northwest Scientific Association to John A. Broussard, Everett Junior College. Research in the human relations files at Univ. of Washington.

The **Damon Runyon Memorial Fund** made the following research grants during March:

Columbia University. D. V. Habif, College of Physicians and Surgeons. Combination chemotherapy of cancer, $15,000.

Columbia University. R. Lattes, College of Physicians and Surgeons. Cytochemical study of nucleic acid and protein synthesis in cultured cells, with reference to the effects thereon of certain antimetabolites, $3500.

Utah State Agricultural College. E. J. Gardner. Genetic and cutological analysis of tumorous head in Drosophila Melanogaster, $2300.

University of Chicago. L. T. Coggeshall. Biological studies of cancer patients who are treated endocrinologically; and topical application of short-lived radioisotopes to inoperable cancer, $20,000.

Duke University. J. W. Beard. Properties of avian leukosis virus, $10,000.

Meetings and Elections

The **Alabama Academy of Science** has elected the following officers: pres., William T. Wilks, Troy State Teachers College; pres.-elect., Ralph Chermock, University of Alabama; sec., Herbert McCullough, Howard College; treas., Lock White, Jr., Southern Research Institute. Patrick H. Yancey, S. J., is the representative to the AAAS Council.

The dates of the **Annual Colloquium of College Physicists** at the University of Iowa are June 16–19. As is traditional with the Colloquium, there will be emphasis on the exhibit of new devices, reports of research, and round-table discussions of teaching problems. Special consideration will be given radio astronomy and the application of atomic power. The program will be climaxed by the four lectures of Dean J. H. Van Vleck of the Division of Applied Science at Harvard, on "Radio and microwave spectroscopy of the solid state."

The **23rd Annual Meeting of the Western Society of Naturalists** was held Dec. 28–30, 1953, at the University of Southern California, Los Angeles. In his presidential address, delivered at the annual dinner, Martin Johnson presented a colorful review of "Tropical and South Pacific islands" illustrating a wide range of highly photogenic natural history topics from atolls to volcanoes. Other evening meetings featured a Hancock Foundation film, "The New Frontier," with a commentary by John Garth; and a special showing at the Walt Disney Studio of "The Living Desert," with an introductory lecture by Mr. Algar. The effective work of the symposium chairmen—Francis Haxo, photosynthesis; John Garth, marine zoogeography; and Ivan Pratt, parasitology; and their participating colleagues—made these symposiums notable. Eight separate sessions were needed for the many papers submitted. Abstracts of these will be supplied on application to the secretary, John L. Mohr, Univ. of So. California, Los Angeles.

The Society plans to join the Pacific Section of the Botanical Society of America in sponsoring—at the AAAS Pacific Division Meeting, Washington State College, Pullman—an afternoon of demonstration papers in genetics on June 24, and to hold a separate morning session the same day for submitted papers.

The fifth annual summer conference sponsored by the Biology Department of Brookhaven National Laboratory, **"A symposium on the thyroid,"** will take place June 9–11; a cordial invitation to attend is extended. On-site accommodations for guests will be reserved in order of receipt of applications up to about 150. Those planning to be present, whether or not they are staying overnight, must notify Dr. Abraham Edelmann, Biology Department, Brookhaven National Laboratory, Upton, L.I., N.Y. *by May 10.* Those who are not citizens of the United States must communicate *by May 1* to allow time for AEC approval of their attendance.

The following scientists are listed on the tentative program: E. B. Astwood and W. P. Vanderlaan, New England Center Hospital; Aubrey Gorbman, Barnard College; S. A. D'Angelo, Jefferson Medical School; C. P. Leblond and N. J. Nadler, McGill University; S. B. Barker, University of Alabama Medical School; Henry Lardy, University of Wisconsin; Jack Gross and P. J. Fitzgerald, College of Medicine at New York City; Alvin Taurog, University of California School of Medicine; William L. Money, Sloan-Kettering Institute for Cancer Research; Hans G. Schlumberger, Ohio State University College of Medicine; Harold P. Morris, National Institutes of Health; Abraham Edelmann, Brookhaven National Laboratory.

The **spring meeting of the American Physical Society** will be held in Washington, D.C. on Apr. 29-May 1, at the Shoreham and Sheraton Park hotels and the National Bureau of Standards. The invited speakers are C. Tobias, R. E. Zirkle, and C. Levinthal in *biophysics*; W. E. Alburger, H. F. Kaiser, R. Sagane, E. Teller, and G. M. Temmer in *nuclear physics*; R. P. Hudson, B. T. Matthias, and D. ter Haar in *low-temperature physics;* R. J. Rubin in *statistical physics*; I. Karle, H. Jones, and R. L. Petritz in *solid-state physics;* H. Lyons, *on velocity of light,* F. N. Frenkiel and O. Laporte in *fluid dynamics*; N. F. Ramsey, H. S. Gutowsky, M. R. Packard, and C. H. Tournes on *chemical applications of radio-frequency spectroscopy*; L. Marton, W. Pines, and L. B. Loeb, and L. H. Fischer, on *electron optics*; C. Wiegand, J. Marshall, J. Tindot, and L. Wolfenstein on *polarization of nucleons by scattering.*

There will also be 31 sessions devoted to 414 10-min papers. At the banquet on Friday evening at the Shoreham Hotel, the speakers will be E. R. Piore and P. Debye.

Nearly 1000 radio engineers are expected to attend the first **Symposium on Global Communications,** which

will be held in Washington, D.C., June 23–25. It will be sponsored by the Institute of Radio Engineers' Professional Group on Communications Systems. Christian L. Engleman, management and engineering consultant of 2480 16th St., NW, is chairman of the meeting. Technical papers on various aspects of worldwide communications will be presented in two full-day sessions by commercial, military, and other Government specialists in the field. Arrangements are being completed for conducted field trips to nearby commercial and military communications centers on the third day of the meeting. It is expected that there will be some exhibits of the latest communications equipment and components. A reception, two luncheons, a banquet, and special activities for the ladies, will also be featured.

The American Institute of Chemical Engineers will hold a **world conference on nuclear energy** at the Univeristy of Michigan, June 20–25. Nuclear scientists from Europe and Asia have been invited to participate in the meeting, and representatives from Belgium, Canada, France, Great Britain, India, Italy, The Netherlands, Norway, Spain, and Sweden have already accepted.

In the United States, the AIChE has secured the cooperation of government, industrial, and educational organizations. Information heretofore restricted has been declassified by the Atomic Energy Commission for presentation at the meeting.

Miscellaneous

The first volume, *Analytical Procedures and Patent Index,* in a series of ozone reference books entitled ***Bibliography of Ozone Technology*** will be published by Armour Research Foundation of Illinois Institute of Technology early in May. The series of six volumes, which will appear over a period of 3 yr, is a comprehensive reference to the literature in the field and is the first work dealing exclusively with ozone to be published since 1918. The series is being compiled by Clark E. Thorp, manager of the chemistry and chemical engineering research department at the Foundation, and his staff.

The various species of fish inhabiting Atlantic waters off New England, New Brunswick, and Nova Scotia are described in a new edition of the bulletin ***Fishes of the Gulf of Maine,*** published by the Department of Interior's Fish and Wildlife Service. Prepared by Henry B. Bigelow and William C. Schroeder of Harvard University and Woods Hole Oceanographic Institution, the 577-page work presents material on the distribution, abundance, life histories, and identification of the fish found in the oceanic bight between Nantucket Shoals, Mass., and Cape Sable, Nova Scotia.

The bulletin revises and adds to Bulletin 40, published between 1925 and 1927 by the Bureau of Fisheries, a parent organization of the Fish and Wildlife Service. The new edition, designated as Fishery Bulletin 74, may be purchased for $4.25 from the Superintendent of Documents, U.S. Government Printing Office, Washington 25, D.C.

Because of the broad interest outside of the fields of fisheries and wildlife ecology, the Wildlife Society has prepared several hundred copies of a symposium on **fluctuations in animal populations** which appeared as part of the January 1954 issue of *The Journal of Wildlife Management.* These are available at $1.00 each from Dr. Daniel L. Leedy, Executive Secretary, The Wildlife Society, U.S. Fish and Wildlife Service, Washington 25, D.C.

Science Reference Sources, a useful guide to more than 1200 representatives bibliographical and reference tools has been announced by the University of Illinois Library School. The list includes general works in engineering, agriculture, and medicine as well as source materials in the pure sciences. A feature of the volume is an outline of the several divisions of modern science with definitions of each subject. Also brought together for the first time is a list of astrographic catalogs covering the heavens from every point on the globe. The work was compiled by Frances Briggs Jenkins, associate professor of library science at the University of Illinois Library School. A limited number of copies is available from the Illini Union Bookstore, Champaign, for $1.00.

The Proceedings of the Annual Meeting, Council for High Blood Pressure Research of the American Heart Association, 1953, has been published as a cloth bound monograph of 96 pages. It may be obtained through the office of the American Heart Association of its affiliates at $2.00 per copy. The monograph consists of a collection of papers summarizing recent investigative work by the authors and their associates on certain aspects of hypertension that is otherwise scattered widely through current literature. The authors are R. W. Sevy, Georges M. C. Masson, Simon Rodbard, D. M. Green, and George A. Perera. Topics covered in this second volume include the relations between hypertension and the anterior pituitary, the adrenal cortex, renin, salt-water balance, sodium metabolism, and electrolyte metabolism. Copies of the first volume of the *Proceedings* (1952) are still available at $1.75 each.

Copies of the complete presentations of the **University of Pennsylvania–American Society of Tool Engineers carbide seminar** held in Philadelphia during the recent ASTE show are now available. The proceedings of the 5-day meeting include the economics of carbide in today's business, tool fabrication and maintenance, single-point tooling results and analysis, outstanding new carbide applications such as milling, gun drilling, trepanning, and detailed data on heat, impact and corrosion resistence. Bound copies with charts, photographs, etc., can be obtained at a cost of $3.00 from Prof. Lee N. Gulick, School of Mechanical Engineering, University of Pennsylvania, Philadelphia 4.

Book Reviews

Recurrent Maladies in Scholarly Writing. Eugene S. McCartney, Univ. Michigan Press, Ann Arbor, 1953. 141 pp. Illus. $2.50.

Despite the somewhat forbidding title, there is not a dull page in these lively essays on the faults of the academic author. McCartney, a classicist turned editor, can quote telling passages from Quintilian to Anita Loos to prove his points. A deft and cheerful surgeon, he accompanies each twist of the scalpel with a jest.

These essays should be required reading for all candidates for the Ph.D. degree and for their preceptors as well; for they cover the whole range and gamut (see p. 36 for the misuse of these overworked nouns) of the boners committed by academic writers.

In his nine chapters, the author considers pedantry, lack of euphony, illogicality, overelaboration, dangling participles, oddities in measuring and counting, misspelling, tautological phrase of specification, and a summary of the general pathology of manuscripts.

The temptation to quote is too strong to be denied, especially since extracts will give the reader a better idea of the book than anything this reviewer could tell him.

In the chapter entitled The Avoidance of Simplicity we read:

> One of our youthful scholars said of a certain place: "Only a small part of all the inhabitants is visually encountered." Would our nation have been more deeply stirred if Perry's laconic dispatch after his victory on Lake Erie had been touched up in similar scholarly form: "We have visually encountered our adversaries, and they are in our possession"?

Discussing infelicities of repetition, he quotes this gem:

> After reaching Greenland the authors reached different conclusions.

McCartney takes a deserved crack at the frequent use of *literally* when *figuratively*, the exact reverse, is meant; and at the misuse of the word *fact*.

> He bought and literally devoured the works of Michael Faraday.
>
> The facts he tells us are few, and subsequent research has shown that they are inaccurate.

In the chapter on overelaboration—appropriately entitled Saying It with Flowers—we meet such specimens as:

> Here unnamed and unanalyzed forces and conditions are interwoven to form a morass which needs exploration and excavation.
>
> Life paid him off with the cruel coin of sour dead sea fruit.

Fine writing, what crimes are committed in thy name!

The useful chapter devoted to the participle contains:

> Being a clergyman and more or less sedentary in my habits, obesity crept on me unawares.
>
> Consumed in excess of 10 per cent of the ration, specialists said poults die of some unknown cause.

In Oddities in Measuring, the disinclination of writers to make straightforward statements is abundantly illustrated. Egregious examples are:

> Thousands of copies of the letter were mailed to every minister and prominent layman in the valley.
>
> The epidermal cells are almost twice or more broader than long.

An alternate title for the volume might be that employed by Harry Leon Wilson a generation ago: *Professor, How Could You?*

PHELPS SOULE

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The Language of Science. Theodore H. Savory. Andre Deutsch, London, 1953. (U.S. distr.: British Book Centre, New York 22.) 184 pp. 10s 6d.

One of the prime responsibilities of the scientist is to communicate his discoveries and opinions. Despite this, as Savory points out in this fascinating book, ". . . it is strange that no one seems to have undertaken a broad study of the language of science." The author thus undertook to begin to fill in the gap that existed between the literature of philology and science. Although written for the layman, this book should be informative and interesting to both philologists and scientists.

Savory notes that scientific writing is essentially cold and informative. As such, it cannot run in double harness with emotive language. Thus, the appeal in the language of science is derived from its authoritative nature.

In addition to assaying the importance of the scientific language, the associated philology is considered. The adoption of Greek and Latin words into English scientific writing is contrasted with the German use of combined forms. The rapid growth of science in the past two or three centuries is shown to be responsible for the simultaneous expansion of the scientific vocabulary. The author suggests that "the man who supplies or suggests a useful name for a new phenomenon, fact or anything else is doing his fellows as real a service as the man who discovers, applies or explains it."

After examining the growth of the language of science, the character of that language is considered. Here, Savory claims, ". . . the writer of the language of science must from the outset abandon all thoughts or hopes of achieving eloquence. . . ." Because of the precision of scientific writing, however, he runs smaller risk of using the wrong word.

Reading this book is a delightful excursion into one of those alluring side roads along the main avenue of science. The main facts are presented with authori-

tative scholarship but with occasional pauses to enlarge upon the features of the language of science that are of interest to the nonspecialist.

The essence of the book is summed up in its final paragraph: "This book has tried to demonstrate the existence of a real language of science and to detect something of its strength and its weakness. Its strength will be enhanced, its weakness will be concealed, and its power for good will become greater as scientists turn their abilities to using it more effectively."

DONALD J. LOVELL

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The Hand-Produced Book. David Diringer. Philosophical Library, New York, 1953. 603 pp. Illus. $15.

David Diringer, author of a previous book entitled *The Alphabet: A Key to the History of Mankind,* has produced a sequel to that work in which he traces the evolution of the written document from the earliest beginnings down to the invention of printing. His title may need further explanation. He is not concerned with the making of books as we know them. Although he himself (p. 24) asks the question, "What is a book?" he does not provide a definition but confines himself to a discussion of the various etymologies. What *The Hand-Produced Book* is really concerned with is the history of written communication.

Dr. Diringer starts with primitive modes of communication, beginning with the first crude drawing that man made in the sand. He then discusses the gradual improvement in communication from the cave drawings and stone carvings to the relatively sophisticated clay tablet books and finally to papyrus books.

It was the discovery of the utility of papyrus that made written communication easy and convenient and made possible the writing of books roughly as we know them. It also gave to European languages their words for paper. Papyrus was a technologic development of the ancient Egyptians who first discovered that the stems of the reedy plants growing in their Nile marshes could be flattened out, glued together in sheets, and used for writing. Papyrus remained an Egyptian monopoly, "but for a thousand years," says the author, "it was the chief writing material for the Graeco-Roman world . . . and was used both for literary and for ordinary purposes such as legal documents, receipts, petitions, notices of birth, and official and private letters (p. 125)."

Parchment and vellum developed naturally from the use of tanned leather as writing materials—how early, no one knows with certainty. But by the second century B.C., parchment, prepared by scraping skins on both the hair and flesh sides and rubbing with pumice stone, had come into fairly common use in Egypt and Asia Minor. The finest grade of parchment vellum, made from calfskin, was used for valued documents. The early craftsmen learned to prepare a particularly fine-grained white vellum from the skin of aborted calves, a type known technically today as uterine vellum. With papyrus, parchment, and vellum as satisfactory writing materials, the ancient world could produce written documents in quantity. During the Middle Ages in western Europe, parchment was widely used for the multiplication of books and other documents.

Diringer's volume provides encyclopedic information about an infinite variety of matters that impinge on writing: the kinds of writing materials used in different countries and regions, the instruments used for writing, including a discussion of inks, pigments, and pencils, and methods of multiplying manuscripts. He ranges over the pre-Columbian Mayas and Aztecs in America, a mysterious riddle that, like the ancient Etruscan writing, remains unsolved.

Many points in the book are controversial, and scholars will not of course agree with all of the author's conclusions, but he provides a stimulating body of information and bibliographical clues for further investigation. Although one of his theses, that the book follows religion, has much to commend it, this is an oversimplification of the complex problem of the reasons for the development of the book. The dust jacket announces that the volume was written primarily "for the cultured layman" and not for specialists. It is not a work however, that one can read easily, for even within chapters it is a succession of sometimes disjointed paragraphs on a wide variety of loosely connected topics. A tendency to repetition and many parenthetical cross references and allusions further retard the reader. But with all of its faults, *The Hand-Produced Book* is both an interesting and useful encyclopedia of information on the written document before the beginning of printing in Europe. The profuse illustrations also add to its value.

LOUIS B. WRIGHT

The Folger Shakespeare Library
Washington, D.C.

Ideologie und Forschung in der Sowjetischen Naturwissenschaft. Schriftenreihe Osteuropa, No. 1. Arnold Buchholz. Deutsche Verlags, Stuttgart, 1953. 126 pp.

Propaganda and counterpropaganda have aroused passions to the point where calm and rational consideration of anything pertaining to the USSR has become a rarity. The book under review belongs to this rare class. The author describes the present situation of science in the USSR and, to some extent, its historical background, with calm and detachment as well as with knowledge and understanding. The book is obviously too short for a thorough coverage of fields as diverse as mathematics, physics, astronomy, chemistry, biology, and agriculture. It is, however, hard to imagine how one could pack more information in every paragraph than the author succeeds in doing. The documentation and references are extensive and, in every instance where the reviewer is competent to judge, accurate.

The evaluations made by the author will assuredly be attacked from the left as well as from the right, since they will appear too severe to some and too lenient to others. In any case, the author did not succumb to the facile generalizations that are so tempting to amateur and to professional propagandists.

Science in the USSR is neither all destroyed nor progressing by leaps and bounds. The situation is more complex than this. In some respects, science enjoys unprecedented opportunities. On the other hand, there is the cancer of Lysenko, which has been nurtured by those in power in blissful ignorance of the fact that he is the most efficient wrecker ever to afflict their biology and agriculture. Furthermore, the situation is fluid, and the future, possibly a very near future, may bring changes and surprises which the author is wise not to attempt to predict.

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Climatic Change: Evidence, Causes, and Effects. Harlow Shapley, Ed. Harvard Univ. Press, Cambridge, Mass., 1953. 318 pp. Illus. + plates. $6.

The title of this volume suggests a meteorological discussion, but only three of its 22 chapters are devoted to meteorology as such. Instead, numerous ramifications of climatic changes, their causes and effects, are discussed. Here one finds such varied topics as radiocarbon dating, tree-ring studies, soil geology, analysis of lake sediments, Pleistocene glaciation, and the relationship of climate to human racial characteristics. Two chapters give excellent and concise summaries of the paleontological and paleobotanical evidence for changes of climate. It would be difficult to imagine a wider assortment of scientific fields, all directly connected with a single main theme.

An introductory chapter by Dr. Shapley includes speculations on the possibilities of life under climatic conditions of other planets. The other 21 authors include meteorologists and climatologists, an anthropologist, several astronomers, a paleontologist, two botanists, a zoologist, and several geologists. The majority are on the Harvard and Yale university staffs. The coordination of chapters is good on the whole, for each author treats a well-defined area that fits into a planned sequence. The lack of a general index is somewhat inconvenient.

Much space is devoted to the problem of the Pleistocene glaciations. The great question of how the vicious circle can start is only slightly less difficult than its logical sequel: once an ice age is established, how can it ever end? Ice begets more ice, as C. E. P. Brooks pointed out some years ago, and some drastic change is required to remove it, once it has taken hold.

Substantial progress has been made since Croll offered his precessional hypothesis, which has been revived more recently in a more precise form by Milankovich. The evidence now seems rather clearly opposed to all such purely geometric astronomical explanations. At best, they can probably account only for minor waves superimposed on the main trend (for example, the variations in the varves of the Green River formation of Eocene age). The geologically rapid alternation of glacial and interglacial episodes is fatal to hypotheses that rely chiefly on elevation of the continents and mountain building. Still, the reviewer finds it hard to avoid the compulsion in the circumstances that two tremendous glaciations (Permocarboniferous and Pleistocene) each followed a few million years after a tremendous orogeny. (Extensive pre-Cambrian glaciations cannot yet contribute clear evidence on this point, owing to difficulties of correlation). It seems at least probable that elevation plays a part in setting the stage.

A recurring theme is the recognition that ice ages represent an accentuation and equatorward shift of climatic zones. It is significant that postglacial times have witnessed cycles that differ only in their shorter periods and lesser amplitudes. All other agencies having failed, the basic cause of world-wide climatic change is considered to be probably solar variation. The naive idea that less radiation would bring an ice age has long been abandoned. Greater radiation is required to increase evaporation and precipitation. But Miss Bell presents the hypothesis in a new form, according to which the earth, especially the oceans, must have been precooled by a cooling of the sun, after which increased activity brought on extensive snowfall. This seems to be the most promising idea yet proposed. Other suggestions concerning effects of solar corpuscular radiation can best be evaluated after we have more definite information. At present we can only regard them as hopeful speculations.

Two decades ago the authors of such a book would probably have felt obliged to refute the hypothesis of continental drift. In this volume it is dismissed quite casually, when mentioned at all. The strongest point in its favor is the Permocarboniferous ice age in and near the tropics, which remains the greatest of all geologic climatic enigmas.

Climatic Change states a problem and discusses its present status but only suggests possible directions in which the solution may lie. There is much in the book that is new and original, to which a brief review cannot do justice. It is well written, far above the "popular" level, and is stimulating and highly informative reading for the scientist or scholar who is not a specialist in climatology.

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Astronomy and Mathematics

Dialogue on the Great World Systems. Galileo Galilei. In the Salusbury translation. Revised and annotated by Giorgio de Santillana. Univ. Chicago Press, Chicago; Cambridge Univ. Press, London, 1953. 506 pp. Illus. $12.50.

Dialogue Concerning the Two Chief World Systems—Ptolemaic & Copernican. Galileo Galilei. Translated by Stillman Drake, foreword by Albert Einstein. Univ. California Press, Berkeley, 1953. 496 pp. Illus. $10.

Galileo's monumental defense of the Copernican system, the *Dialogue on the Two Principal World Systems,* has been virtually inaccessible in English since the Great Fire of London destroyed most copies of Thomas Salusbury's 17th-century translation. Until 1953 the fire damage was not repaired. Most English and American readers have known Galileo only through his *Discourses on Two New Sciences.* Now two English editions appear simultaneously: one, a brilliant revision of Salusbury's clumsy and inaccurate translation; the other, a completely fresh

translation from the Italian original. Both are admirably clear and faithful versions of one of the few readable classics in the literature of science.

The Galileo who emerges from the *Dialogues on the World Systems* is a less familiar scientific type than the Galileo of the *Two New Sciences*. The latter appears as a theoretical engineer, concerned with a limited range of practical terrestrial problems; he can be drawn as the "first modern scientist," the man who destroyed medieval science at a stroke by insisting upon the supreme authority of observation and who founded modern science by the experimental solution of its first fundamental problems. The author of the *Dialogues* will not conform to this mold. He is a speculative Renaissance cosmologist who, with unrivaled virtuosity, will turn every observation and trick of dialectic to the documentation of his vision of the universe as uniform and homogeneous. His first concern is to banish from cosmology the primitive perception, dominant in most ancient and medieval science, which divides the universe into two intrinsically disparate portions: the corruptible and chaotic earth, and the eternal and regular heavens.

The vision of the uniformity of matter and laws ties the *Dialogues* together and is the key to much of its substance and originality. Galileo is the first scientist to fruitfully apply celestial observation to the discovery of terrestrial laws. The eternal regularity of the heavens demands that the celestial bodies move naturally in circles; the universe would dissolve, says Galileo, if natural (inertial) motions were linear. To this point, he is a good Aristotelian. But for Galileo the same natural motions must be exemplified on earth. It is only a naive trust in uninterpreted sense-data that misled Aristotle to assert that the unconstrained motions of terrestrial bodies were straight. In fact, Galileo says at one point, the stone that appears to fall straight along the side of a vertical tower with steadily increasing velocity is really governed by the same law of uniform circular motion as the planets. It really moves uniformly in a semicircle from the top of the tower to the center of the earth, but the earth's diurnal rotation keeps the tower aligned with it throughout the fall. These ideas are not Newton's laws, but they are Newton's vision. The celestial moon and the terrestrial apple display identical aspects of nature's fundamental regularity.

To many readers, the most startling characteristic of the *Dialogues* will be the attitude displayed toward exact observation. The book is filled with an unparalled wealth of qualitative observation, brilliantly interpreted to demonstrate Galileo's predetermined theorems. Galileo is a master of the simple demonstration technique; there is nothing to be observed in nature so trivial that he cannot turn it to account. And this is a key novelty of his work. But he does use nature to demonstrate, not to derive, and he does not hesitate to suppress troublesome details again and again. The pendulum, whose circular motion is for Galileo the key to so many of nature's regularities, is for him absolutely isochronous. He has watched, he says, two bobs on strings of the same length swing back and forth, remaining together: one had a small amplitude, the other swung through nearly 180° of arc. More essential suppressions occur in the discussions of astronomy. Galileo restricts himself entirely to a simplified version of the Copernican system, a version that could satisfy no astronomer, including Copernicus, because it was unable to account for the observed planetary motions as well as Ptolemy's system did. But Galileo's faith in the geometric simplicity of natural law was stronger than his faith in quantitative observation, and the astronomers' fuller quantitative version of the Copernican system, with its epicycles and eccentrics, would not conform to Galileo's system of circular inertia.

The Galileo of the *Dialogues* is a far more complex figure than the more familiar Galileo of the *The Two Sciences*, and the *Dialogues* is a correspondingly richer and more rewarding book. The unfamiliar facets of Renaissance scientific thought which emerge in this book are ingredients essential to an understanding of the intricate fabrication linking the critical and analytic science of the later Middle Ages to the more empirical and mathematical Newtonian science of the later 17th century. Either of these new editions will illuminate the sources of Newtonian science. Perhaps the Chicago edition, with a more readable format and with fuller and more perceptive notes, will facilitate more penetrating insights.

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Principles of Numerical Analysis. Alston S. Householder. McGraw-Hill, New York–London, 1953. 274 pp. $6.

During the past few years, the number of people working in the field of numerical analysis has increased sharply. This increase is due to the growing use of applied mathematics in the research laboratories of industry, government, and universities. It is also due to the introduction of new instruments for computing, namely, electronic digital computers. These factors have led to a development of the field and a need for books that treat the subject as it is used today. *Principles of Numerical Analysis* treats many topics not adequately covered by the classical textbooks on the subject.

The book begins with a chapter on computation and a discussion of errors. The remainder of the book is concerned with a mathematical analysis of the general classes of problems met in performing calculations. First, there is a discussion of linear algebraic equations, which, as the author notes, are used extensively in almost every calculation. Differential equations, integral equations, and many other problems can be reduced to sets of linear algebraic equations for calculational purposes. Next, nonlinear equations are discussed, followed by a chapter on eigenvalue problems. Other chapters treat interpolation, general methods of numerical integration, and differentiation. The final chapter presents a brief discussion of the Monte-Carlo method.

This book presents a good discussion of errors in calculation. Errors, the changes in the results owing to the finite number of digits used and owing to the representation of a continuous problem by a discrete problem, are distinguished from blunders, actual mistakes in calculation.

The mathematical aspect of numerical analysis is the principal topic of the book. The author attempts to give the reader some insight into the mathematical problems involved in carrying out numerical calculations. This subject has been neglected in most preceding textbooks, and its emphasis here points out the increasing mathematical interest in calculational methods.

It would be desirable to see more examples of calculations in a book of this type. The important problem of stability of numerical calculations is neglected, and there is no explicit treatment of the numerical solution of differential equations. The use of the new computing tool, electronic digital computers, is not discussed. These omissions suggest that there is a need for another book that would be concerned with them.

E. C. NELSON

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Astronomical Photoelectric Photometry. Symposium presented on Dec. 31, 1951, at the Philadelphia meeting of the AAAS. Frank Bradshaw Wood, Ed. American Association for the Advancement of Science, Washington, D.C., 1953. 141 pp. Illus. $3.75.

Assembled in this book are eight papers presented at a symposium sponsored by the AAAS at Philadelphia. The purpose of the symposium was to outline the status of methods and techniques then in use and under development for astronomical photoelectric photometry. The contents of the book indicate that the purpose of the symposium was served admirably, and the book is a useful and available permanent record of the results.

There are papers by nine authors, each an authority in some particular aspect of the field. This number of authors, to say nothing of the large bibliography, is a good indication of the growth of the art since the pioneering work of Joel Stebbins began 40 years ago.

The first chapter, by Albert P. Linnell, on the use of direct-current techniques, covers the methods in general routine use for the bulk of successful photoelectric observing in most observatories. Following this, John Hall discusses alternating-current techniques and the special conditions under which they are particularly useful. Hall also gives a good discussion of the effects of stellar scintillation, of which he has made a special study.

William Blitzstein summarizes the methods, advantages, and disadvantages of photometry by counting pulses from individual electrons emitted by the cathode of a photomultiplier, and also gives descriptions of actual and projected installations of counter photometers at the Flower and Cook Observatories. Counting methods are considered further by R. O. Redman and G. G. Yates in another chapter. These authors describe the development of two counting-type instruments at Cambridge, England, and provide a good discussion of methods for extending the linear operating range of counter photometers and of correcting for the nonlinearity in a portion of the nonlinear range.

Two short chapters by the French authors A. Lallemand and F. Lenouvel describe, respectively, the characteristics and use of a 19-stage photomultiplier constructed by Lallemand. This multiplier has enough multiplying ability so that shot noise can be observed for an unrefrigerated cell with a glavanometer. Useful photometry has been done with the instrument at the Haute Provence Observatory. Certain precautions must be exercised to prevent fatigue effects in this multiplier, in which rather high current densities can prevail.

Th. Walraven discusses his developments in the use of servomechansims for providing a logarithmic output from a photometer. In this way a photometer, with the sacrifice of simplicity, can be made to read directly in magnitude instead of in light units.

The last chapter by A. E. Whitford is an able and realistic summary of the fundamental principles involved in the photometry of faint light. Although the chapter is quite properly placed last in view of its intended function, students of photometric techniques may well read it first for orientation.

All chapters contain useful data, equations, circuit diagrams, and descriptions of equipment and methods, in some cases in considerable detail. The book is valuable in that it presents a large variety of methods for the perusal of a prospective user, who can give consideration to all of them and pick the best for his particular application. The book also presents a fine bibliography, particularly in the case of Linnell's paper, which gives nearly 600 references.

There are a few omissions and inconsistencies, as might be expected, as a result of assembling papers by separate authors. There are also a few mistakes, of which one or two are rather unfortunate. Most of the flaws are readily recognizable and therefore unimportant. A person who would read this book from the viewpoint of 20 years ago would be surprised at the complexity now shown by techniques once thought to be complicated by Whitford's introduction of the one-tube amplifier.

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The Sun. The Solar System, Vol. I. Gerard P. Kuiper, Ed. Univ. Chicago Press, Chicago, 1953. 745 pp. Illus. $12.50.

For many years, astronomers and other scientists interested in solar physics have longed for an up-to-date, unified textbook that covered in a systematic way the various phases of this growing field of research. *The Sun,* the first of a series of four books on the solar system, fills this need in excellent fashion. Nine authors contributed to the main sections of the book, and 13 additional authors contributed to the final chapter, Empirical Problems and Equipment. The book was written for readers trained in the physical sciences and

will be of great value to anyone interested in a comprehensive description of solar phenomena and in a thorough review of the theories that have been developed to explain them. More particularly, it will be greeted enthusiastically by those actively engaged in solar and solar-terrestrial research. It certainly will immediately become a standard reference textbook for this field of scientific investigation.

The Sun is a praiseworthy book from several standpoints. For the most part, the presentation is well organized and easy to read. The printing is excellent, and the text is liberally illustrated. The authors were careful to summarize and give credit to the work of others. The book gives extensive and well-chosen references. In general, there is a clear distinction between the observational data and the authors' interpretations of the data.

Some chapters are especially well done. Particular praise should go to Bengt Stromgren's discussion of the problems concerned with the solar interior and to M. Minnaert's chapter on transfer of radiation and the formation of the spectrum. These are both unusually valuable surveys of the current knowledge and state of research on these fundamental problems. The chapter on solar activity by K. O. Kiepenheuer is thorough, and the author has injected many interesting suggestions regarding the interrelationships among the various phenomena.

The chapter on solar emission in the radio wavelengths by J. L. Pawsey and S. F. Smerd is the most satisfactory summary to date of the observational data in this interesting new field. T. G. Cowling has surveyed the many and varied interpretations of solar phenomena in terms of interacting charged particles and magnetic fields. His main contribution, aside from the unified review, is his criticism of existing theories through order of magnitude arguments. For many theories, his arguments appear to be fatal. However, the ranges of possible values of the parameters, on which all theories of this type depend, are so broad that even order of magnitude arguments are in some cases inconclusive.

The chapter on the chromosphere and corona by H. C. van de Hulst requires special comment. In the author's own words, "To study the corona and chromosphere requires a firm optimism." The author has done a masterful job with a very difficult problem. This is especially true of his treatment of the corona. His surveys of the chromospheric data and the work of others in interpreting the data are excellent. Unfortunately, his own interpretation of the data in terms of a "model" chromosphere is not convincing. Specifically, he has combined the various data in a very arbitrary manner that obscures some of the most basic facts given by these data. He then proceeds to derive a "model" chromosphere by a method that is based on what seems to us very dubious assumptions.

We have the feeling that the discussion of the identification of solar lines, in Chapter 4, devotes too much space to discussions of elementary laboratory spectroscopy and not enough space to the important research problems in this field. The general unity of the book is also slightly marred by the adoption of a different system of citing references in this chapter.

Chapter 3 contains a few obvious typographical errors in both mathematical equations and the text. A statement in the section on eclipse problems in Chapter 9 to the effect that the chromosphere disappears behind the eclipsing moon in "3 or 4" seconds might more appropriately have said "20 seconds or more," even though it is true that to the naked eye the chromospheric "flash" seems to last only 3 or 4 seconds.

The editor is to be complimented for including the valuble chapter on Empirical Problems and Equipment and also the tables giving locations and types of observations of all solar observatories throughout the world.

The Sun is obviously of first importance in solar research and fills a long-standing need of investigators in many fields of study.

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The Elements of Mathematical Analysis, Vols. I and II. 2nd ed. J. H. Michell and M. H. Belz. Macmillan, London, 1950. 1087 pp. Illus.

In the preface to the first edition (1937), the authors said:

> In writing the present book we have tried to make it conform to three main conditions. . . . (i) The book should assume as known only those elements of Algebra, Geometry and Trigonometry which are taught in the secondary schools to all those preparing to attend any lectures in Mathematics at a university. . . . (ii) The second main condition is that the book should form a practical or working text provided with an abundance of illustrative examples treated at length and of other examples to be solved by the student. . . . (iii) The third condition is that the subject should be expounded on the basis of the theory of real numbers, geometrical notions being employed only illustratively and not as replacing abstract discussions. . . .

These stipulations and principles apply to the second edition also, for the new preface states that, apart from corrections and improved modes of expression, no other substantial changes have been made.

The authors have succeeded admirably in meeting their aims. With regard to the first condition, it is a question, of course, of how well the necessary comprehension and facility in the use of the assumed "elements" have been ingrained in the student. If no obstacles of inadequate preparation or ability interfere, the second condition can also be said to have been well fulfilled. The illustrative examples have been carefully chosen and painstakingly expounded, and the student should benefit further by independent work with the equally excellent exercises and problems.

The third condition is fulfilled also in a manner consistent with the first one. As an indication of the book's level, the appearance of the Bolzano-Weierstrass theorem as early as page 15 may be cited. But, in fairness to the authors, it should also be mentioned that their preface includes the sentence:

> If, nevertheless, any student finds the first Chapter too heavy to read as a whole, he may well be guided to postpone the reading of various discussions until they are appealed to in the sequel.

Although mathematical precision is almost always

maintained throughout the book, there are a few lapses that may be misleading or are lacking in rigor. On page 6, for instance, it is a bit of a jar to read that "0/0 may be any number, or is indeterminate." Also, on page 95, in connection with a discussion of orders of infinitesimals (which could easily have been avoided entirely, and should have been), one finds the statement that "an infinitesimal of the nth order is ultimately indifinitely small compared with one of the $(n-1)$th or lower order."

Several topics not customarily seen in similar treatises are considered here, such as finite differences, line coordinates, least squares, and orthogonal functions. An interesting feature is the inclusion of epicene and expocyclic functions, "thereby avoiding the introduction of complex numbers in the treatment of the elementary theory of functions of real variables" (p. 443), and similarly using these real functions when dealing with linear differential equations in Chapter XX.

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Physics

Scientific Papers Presented to Max Born. Sir Edward Appleton *et al.* Hafner, New York, 1953. 94 pp. Illus. $2.50.

This "modest volume of essays," as it is accurately described in the foreword, was prepared last year as a tribute to Professor Born upon his retirement at a vigorous 70 years from the Tait Chair in the University of Edinburgh. In 10 brief essays the contributors, most of them old friends and colleagues of Born, treat a variety of subjects ranging from the ionosphere to the theory of algebraic fields.

The hero of the work appears himself only in the lines of a formal bibloigraphy that somehow becomes about as interesting as many a scientific paper. Almost 300 papers by Born and his coworkers are listed, and nearly a score of books, two still gaining form in his fruitful hands. If, like this reviewer, you owe much to Professor Born's books, to the *Restless Universe* in its charm and depth, to the compendious, *Optik,* to the meaty *Atomic Physics,* or to any of the others, you will be especially struck by this list. But anyone may see in the titles a kind of précis of the physics of our century. Here are named the elegant early exploitations of special relativity 40 years back, the complex and powerful theory of lattice vibrations, the studies on collisions in which that essential first bridge to understanding, which we now call the Born approximation was built, the adiabatic approximation, still the heart of the theory of molecular structure, and many more. There are less familiar matters, too, such as the boldly nonlinear electrodynamics and the still hotly discussed kinetic theory of liquids. All these are a good harvest indeed.

About 30 years ago Born set one stone which has become the builders' chief cornerstone. Hamiltonians and wave equations may give way to state vectors and path integrals, but the statistical interpretation of the probability amplitude remains the foundation of every quantum theory. It is fitting and proper that four of the 10 papers of the present book, papers by Bohm, de Broglie, Einstein, and Landé, in three tongues, all address themselves to this general question. Once again Professor Einstein acutely questions the completeness of quantum mechanics, because (if I may peremptorily summarize his careful thought) it cannot lead to an unambiguous classical limit of specific, and not probabilistic, description. He dismisses the use of the wave packet, it seems to me, too lightly, on the grounds of its finite duration. It is interesting indeed that the makers of casual alternatives to quantum theory, Bohm and de Broglie, earn from him the same blame because their theories make a particle in a well stand still, while classical physics gives it a velocity, and the ordinary quantum theory of stationary states only a probability ensemble of two possible velocity values. Their answers contain much of what a quantum mechanician would say in his own defense. Landé has a thoughtful paper again emphasizing the naturalness of the statistical theories.

The other papers range from a specialized piece on the theory of flame propagation by von Kármán and S. S. Penner to a conjectural little note by P. Jordan on fundamental biology. Courant, Schrödinger, and Weyl are other contributors whose names serve to remind us again how great a debt we all owe to those Göttingen years of decades back. Sir Edward Appleton, the one experimenter, writes his views on the interaction of the ionosphere with the earth's magnetic field and speaks as the spokesman of Edinburgh, which Professor Born has served and graced for 17 years.

As a frontispiece there is a signed photographic portrait of Professor Born, elegantly made by a London photographer, whose name, Lotte Meitner-Graf, is not without interest for physicists. The fabric of the history of physics in our time is closely knit.

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Thunderstorm Electricity. Horace R. Byers, Ed. Univ. Chicago Press, Chicago, 1953. 344 pp. Illus. $6.00.

Thunderstorms are the most spectacular electric manifestations of the atmosphere. In 1912, C. T. R. Wilson suggested that they acted as the generators which maintain an electric charge on the earth in the presence of the considerable current conducted by the atmosphere. Although this hypothesis has met with considerable favor ever since, it is only in the last few years that substantial experimental evidence in its support has been available. It is, therefore, an appropriate time to gather together the known facts on the electric behavior of thunderstorms and (to quote the editor's preface) "bring some order out of the chaos of facts on thunderstorm electricity." This was the object of a conference held at the University of Chicago in April, 1950, and sponsored by the U.S. Air Force Cambridge Research Center. This book makes available the material presented at that conference, together with some results obtained since that time.

It thoroughly covers the subject of thunderstorm electricity and spreads into many neighboring fields, such as cloud thermodynamics, the charging of dust

particles, propagation of atmospherics, ice-crystal growth, and the more universal aspects of atmospheric electricity. There is scarcely an experimental fact of any importance on the subject that is not covered either in the text or through the references. Most of the important recent experiments are described in detail, many by those who performed them.

Most of the book is devoted to field observations and laboratory experiments directed toward determining the mechanism by which a thundercloud charges certain atmospheric particles positively and others negatively and separates these to form a positively charged upper part and negatively charged base. The number of different mechanisms considered and the divergence of lines of experiment show that no proposal has been universally accepted as yet.

The sixteen chapters have been written by 20 authors, all specialists in at least one phase of the subject. The first two chapters serve to place the subject in the perspective of atmospheric electricity as a whole. Chapters 3 and 10 describe potential gradient and conductivity measurements in relation to thunderstorms. Chapters 4 and 5 review the meteorology of thunderstorms and the properties of hydrometeors. Then follow four chapters principally devoted to charging mechanisms that may be of importance to the electric phenomena of thunderstorms, and observations on the disposition of charges in thunderclouds. Chapters 11–13 deal with lightning from the point of view of the electrostatic and radiation (that is, "atmospherics") fields it produces, with the following chapter devoted to the location of thunderstorms by this means. The last two chapters cover the more applied fields of the prevention of lightning damage to aircraft and power lines.

One serious omission is the absence of an adequate description of the technique and principles involved in radar mapping of thunderstorms. Its inclusion would have made many of the statements of Chapter 4 more meaningful to those not familiar with the capabilities of this method.

The book is liberally provided with diagrams and photographs, all of high quality. In several diagrams, ambiguity is introduced by the crossing of solid lines. Misprints are rare, but they have an annoying habit of appearing in formulas. In Chapter 2, for example, "W" and "w" are used interchangeably for the specific resistance. References to diagrams are sometimes misleading, for example, the abscissa of Fig. 12 of Chapter 7 is described as being on a 3/2 power scale, whereas actually both abscissa and ordinate are on logarithmic scales. The use of *Thunderstorm Electricity* as a reference book would have been enhanced by the addition of an index.

The reader who is looking for a simple and definite explanation of the mechanism of charge generation in thunderstorms will be disappointed. But when such an explanation is forthcoming, its author will doubtless owe a debt of gratitude to this book.

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Dislocations and Plastic Flow in Crystals. A. H. Cottrell. Oxford Univ. Press, New York, 1953. 223 pp. Illus. + plates. $5.

Dislocations in Crystals. W. T. Read, Jr. McGraw-Hill, New York, 1953. 228 pp. Illus. $5.

Two excellent books devoted to dislocations have now appeared. Fortunately the two complement each other nicely.

In most cases, a wealth of experimental observations stimulate the induction of a set of basic principles that may be used to give a concise description of the entire field of phenomena. Once the basic principles are known, one can travel the reverse road and use them to deduce the results to be expected in specific circumstances.

Dislocations were invented to provide an understanding of the behavior of crystals during permanent deformation. Unfortunately, there are still several aspects of the deformation problem that are not understood.

Cottrell leads one into the dislocation concept by this historical path describing the observed geometric nature of the slip process in crystals and showing how the low observed shearing strength leads naturally to the type of crystal imperfection called a dislocation. Following a geometric description of the nature of the lattice distortions around an edge and a screw dislocation, Cottrell describes the elastic stress systems that exist in materials containing dislocations. These elastic stresses which extend to large distances lead naturally to his discussion of the interaction forces between dislocations and the interaction force between an impurity atom and a dislocation. The effects of the periodic lattice structure are then considered, and, among other things, partial dislocations, crystal growth, the Frank-Read dislocation source, and the dislocation model of grain boundaries are treated.

In the remaining two chapters, Cottrell gradually leads one from situations that are well understood to cases in which even the data are suspect, to say nothing of the theories. In the chapter dealing with the yield strength, there are certainly various aspects of the theory and of its relation to experiment that are well founded. For example, consider Cottrell's own theory of the upper yield stress. By now, this is a beautiful theoretical development well checked by experiment. On the other hand, very recent data by Blewitt and coworkers (published since Cottrell's book) refute certain aspects of the ideas presented concerning slip in solid solutions. Other similar cases can be found in the last chapter on work-hardening, annealing, and creep. Cottrell himself gives fair warning regarding the uncertainties in the last chapter. Some individuals might object to a discussion of a field that is as yet not well worked out, but actually this is where the future fun is to be had. Anyone, but particularly those not well versed in the field of dislocations and plastic flow, will welcome Cottrell's well-balanced account of the present incompete state of our knowledge concerning the mechanical strength of crystals.

In *Dislocations in Crystals,* Read to a large extent adopts the deductive approach. He introduces the reader to the dislocation as a type of flaw in the crystal structure. His first seven chapters give principally a clear and well-diagramed geometric exposition of the nature of the crystalline disturbance in the vicinity of one or more dislocations. It is true that the concepts of the force on a

dislocation and that of the line tension are introduced, but the main emphasis is on the geometry. The generation of new dislocations by the Frank-Read mechanism is carefully pictured. Chapter 7 gives a clear and rather complete discussion of the geometric nature of the particular dislocations and stacking faults appropriate for a given crystal structure. Thus far in the book practically no quantitative experimental observations are mentioned; a small number of qualitative observations are used to guide the theory. This is in contrast with Cottrell, where order of magnitude estimates appear frequently and from the beginning.

Read begins active quantitative calculation in Chapter 8, which deals with the elastic stresses produced by dislocations and with the associated elastic energy. Chapter 9 is concerned with the forces acting between dislocations and with a qualitative discussion of the anchoring of dislocations by impurities.

The second portion of the book applies the theory to two problems in which the theory makes definite predictions that have been verified by experiment. Read shows in the case of the growth of crystals from the vapor or from solution that Frank's dislocation theory (i) predicts the observed steps on the growing crystal face and (ii) correctly predicts the degree of supersaturation required. Read and Shockley's theory of the grain boundary energy fits observations on five or more metals and contains only one adjustable parameter. In addition, the possible motions of a grain boundary are discussed.

To summarize, Cottrell probably gives a more well-balanced picture of the present status than Read. Read, by concentrating on partial dislocations, crystal growth, and on the grain boundary energy is able to give a very thorough, lucid treatment of these fields.

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Radioactive Isotopes. An introduction to their preparation, measurement, and use. W. J. Whitehouse and J. L. Putnam. Oxford Univ. Press, New York, 1953. 424 pp. Illus. $10.

The authors of this book are to be commended for presenting a concise and lucid account of the physical background—a knowledge of which is helpful to all who use radioactive isotopes as research aids. They have imposed the requirement that the text deal with material sufficiently general to interest an audience of widely varying background while avoiding overspecialization.

There are 8 chapters. The first three provide a short summary of nuclear reactions, modes of nuclear disintegration, and properties of the radiations encountered in using radioactive isotopes. Most of the space is allotted to the next three chapters, which deal with production of radioactive isotopes and detection, measurement, and gross effects of the radiations. These chapters are particularly well organized. There are two final chapters, one a short but well-condensed exposition of applications, and the other a general treatment of problems involved in manipulation of radioactive material. There are also an interesting historical introduction and four appendices listing physical constants, isotope masses, thermal neutron capture cross sections of the elements, and an abridged isotope table. Finally, there are name and subject indexes.

According to the authors' preface, the preparation of this book was begun in 1948. Apparently, it was not finished until 1951. Two years more passed before publication. Consequently, despite the publication date of 1953, the material covered relates only to work published before 1951. Both the advantages and disadvantages of this leisurely schedule are apparent. Thus, the format is exceptionally good, and the text shows evidence of careful editing. On the other hand, a considerable fraction of the material presented seems somewhat transitory for crystallization in such an excellent (and expensive) format.

The present volume, together with others in the series of textbooks on associated subjects published by Oxford, demonstrates that know-how in the application of nuclear physics to all the sciences and to engineering is as well developed in England as anywhere in the world.

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Principles of Transistor Circuits. Richard F. Shea, Ed. Wiley, New York; Chapman & Hall, London, 1953. 535 pp. Illus. $11.

This book, which represents a cooperative effort of the engineering staff of the General Electric Electronics Laboratory, surveys the mushrooming field of transistor circuits. Any attempt to cover such a rapidly growing subject is bound to be a bit blurred in spots and is faced with the problem of obsolescence sooner than a text in an established area. In this case, the authors were faced with the additional problem of producing a volume that would be of value to both students and established engineers. In the interest of the later group, analogies between vacuum tube and transistor circuits are emphasized wherever possible. This point of view restricts discussion of some of the very interesting and potentially useful circuits, such as those possessing complementary symmetry. On the whole, however, this book represents a good summary of the state of transistor circuit theory at the time of its publication, and it should serve as a useful introductory volume for some years to come.

After a brief chapter outlining semiconductor principles, the authors devote five chapters to the transistor, as a linear circuit element, which can be completely described by measurements on its external terminate. T network small-signal equivalent circuits are derived for the grounded emitter, base, and collector configurations in Chapter 3; these are applied to the analysis of single stage and then multistage amplifiers in Chapters 4 and 5. The practical problems of maintaining quiescent operating points, despite the marked variation of transistor characteristics with temperatures, are treated in Chapter 6 under the heading Bias Stabilization. In this reviewer's eyes, the only weakness of the "small-signal" portion of the book is a failure to emphasize some of the other very useful equivalent circuits that can be employed. This is particularly true in Chapter 9 where the properties of transistors at high frequencies are considered. The choice of a fixed parameter equivalent circuit, which can best predict the transistor performance at high frequencies, is an active problem at the present time; it

seems desirable, therefore, to maintain a flexibility of thinking in this regard, particularly in view of the very considerable limitations of the T network equivalent.

Chapters 10–13 consider small-signal design of high-frequency amplifiers and oscillators. This section discusses in detail some of the useful coupling schemes for band-pass and video amplifiers. The lack of a satisfactory high-frequency equivalent circuit and the importance of the "built-in" feedback of a transistor makes the value of some of this analysis questionable. Qualitative results can be obtained but little more. The discussion of oscillators is extremely meager.

In Chapter 14, the authors do finally point out some of the more general approaches to transistor circuits, but unfortunately many readers may never be aware of this since the material is introduced as an illustration of matrix analysis, a completely unnecessary sophistication.

Large-signal analysis of conventional amplifiers is presented in Chapter 7, and switching circuits are treated in Chapter 19. The power amplifier discussion concentrates on the grounded-base configuration, despite the equal importance of the grounded-emitter and collector configurations.

Despite the individual points criticized here this reviewer feels that *Principles of Transistor Circuits* is, on the whole, a good book. The material presented is readable, and it should serve the avowed purpose of introducing engineers familiar with vacuum-tube circuits to some of the possibilities and idiosyncrasies of transistor circuits.

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Chemistry and Biochemistry

Present Problems in Nutrition Research (in German, English, and French.) *Experientia*, suppl. I. Proceedings of the symposium held in Basel, Sept. 1–4, 1952, under the auspices of the International Union of Nutrition Sciences. F. Verzár, Ed. Verlag Birkhauser, Basel–Stuttgart, 1953. 312 pp. Illus. Sw. fr. 32.

Nutrition is unique among scientific disciplines for its catholicity. In its study of the requirements, digestion, utilization, and metabolic fate of essential and nonessential nutrients, it coincides with much of biochemistry and physiology and touches some parts of microbiology. In its quantitative aspects, it can be superimposed upon bioenergetics and with some aspects of environmental physiology. Because of its implications in nutritional, as well as degenerative, diseases and its relation to the problem of resistance to infection, it is of growing importance in medicine and public health. It is an essential basis of animal husbandry. Because nutrients must be translated into foodstuffs, food chemistry is an integral part of the science of nutrition. Because, in turn, these foodstuffs must be produced, procured, economically available, palatable, and acceptable, the nutritionist must possess some degree of familiarity with agriculture, canning, milling, and refrigeration techniques, and with the branches of economics dealing with the production and distribution of foodstuffs as well as with the buying power of consumers. He must know something of the statistical methods used in population studies, be acquainted with known facts on the psychology of taste, and recognize social and religious tradition determining food habits as well as legal practices governing enrichment and addition of chemicals to foods. Truly, the nutritionist can apply to himself the verse of Terence: *Humani nihil a me alienum puto.*

This universality is strikingly reflected in the volume summarizing the proceedings of the Symposium on Present Problems in Nutrition Research that was held in Basel. This book, ably edited by F. Verzár, presents some 20 talks given by well-known European nutritionists as well as the discussions that followed each talk. Particularly outstanding, in the opinion of this reviewer, were the presentations of Tremolières (Surveys of Food Attitudes and Habits); Cuthbertson (Microbiology of Digestion); Bigwood (Free and Combined Amino Acids in Foodstuffs); Karl Thomas (Utilization of Synthetic Fats); Abramson (Chemicals in Foods and Their Control by Health Authorities); Dam (Vitamin E as an Antioxidant); Folley (Practical Possibilities of Use of Hormones in Nutrition); Beznak (Relation of Dietary Fat to Work and Growth); and Demole and Cremer (Present-day knowledge of Dietary Role of Fluorine and Other Minerals). W. R. Aykroyd, the director of the Nutrition Division of the Food and Agriculture Organization, gave a lucid and comprehensive review of the nutrition work of FAO. A. G. van Veen, also of FAO, discussed the question of satisfactory protein sources for supplementary child-feeding programs, a point of great importance now that Kwashiorkor (protein deficiency syndrome) is emerging as the most urgent world-wide nutrition problem.

The book, attractively printed, represents an excellent cross section of present-day problems and is a valuable addition to the nutritionist's library.

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A Simple Guide to Modern Valency Theory. G. I. Brown. Longmans, Green, London–New York, 1953. 174 pp. Illus. $2.50.

Although this book was originally written by a master at Eton for use in English public (that is, private) schools by "advanced sixth form pupils," many chemistry students in this country, both undergraduate and graduate, as well as older chemists who wish to keep abreast of modern developments will find this slim volume of great usefulness. In a simple, mainly qualitative manner and with a minimum of mathematics, the author has presented a clear and concise account of modern valency theory.

A short historical introduction, tracing the development of the concept of valency from Berzelius and Dumas to the present, is followed by an outline of atomic structure, including a simply written but excellent chapter on the arrangement of extranuclear electrons. This is succeeded by a discussion of electrovalent, covalent, and

dative bonds, a detailed treatment taking up approximately half the book. Following this is a brief account of the experimental methods that support the theoretical development, simple discussions of resonance and hydrogen bonding, and finally a short chapter on molecular orbitals.

This book will serve as a much-needed bridge between the treatment of valency as it is now covered in undergraduate chemistry textbooks and the detailed, highly mathematical presentation in advanced works devoted entirely to the subject. As such, it is a real contribution to a more complete understanding of chemistry.

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Chemistry of the Lanthanons. R. C. Vickery. Academic Press, New York; Butterworths, London, 1953. 296 pp. Illus. $6.

Here, from the land of the leaping kangaroo and the fragrant eucalyptus, comes a worth-while book on the increasingly important rare earths, albeit under a banner carrying the somewhat lofty inscription "lanthanons." This new designation, proposed by J. K. Marsh, of England, is used in place of the traditional "rare earths," La to Lu, on the grounds that such elements are neither rare nor found only on our own spinning globe. Without wishing to stem progress or to thwart those eminent chemists who deem another more enchanting name to be desirable, this reviewer respectfully submits, in the interests of humble reason, that lanthanon comes from a Greek word meaning *escape notice*. Aside from this, lanthanon, as the author uses it, almost implies that the lanthanum in Australia has mystical pseudoscalar tentacles that keep it in constant touch with other rare earths in Brazil, Madagascar, India, Idaho, and elsewhere. But we are not here to curse a new name and the learned gentlemen who propose it, but rather to describe and praise the author's book.

To begin with, there is a fascinating chapter on the history associated with the discovery and separation of the rare earths. Both here and abroad, many of the noted chemists of the past, and in our own times, have had a hand in the often tedious work of discovery and isolation of these curious elements. Oddly enough, the uncertainty regarding their total number, and the effort spent on them, revolved for many years about the mistaken notion that each light absorption band indicated one element, "one band—one element"; an erroneous theory can sometimes be more fruitful than a correct one. During the last 15 decades or so, there were acrid disputes, evidences of coercion, and even sealed packets containing unrevealed information; there is an old south-of-the-border air about it all, and one finds it easy to imagine Mosander working feverishly, while Berzelius presses an unrelenting pistol to his back. *Tantaene animis caelestibus irae?*

The second chapter deals with the composition and occurrence of various minerals that contain the rare-earth elements. (Bastnasite is practically ignored.) Here one finds very informative graphs showing atomic abundances and mineral compositions. Stony meteorites and the sun contain several of the strange elements. Chapters 3 and 4 describe the principal experimental and theoretical studies of a physical nature made by chemists as well as by distinguished physicists. Valence states, magnetic properties, absorption and emission spectra, isotopic constitution, and electronic states are discussed briefly but still with enough detail to set the beginner, the amateur, and the professional on the right course. We are now up to page 64.

Then come several chapters on the many separation procedures, analytic methods, chemical properties of individual compounds when known, and on practical applications. These are matters close to a real chemist's heart, and, considering the wide variety of separation methods and techniques, each with its one or more virtues, the author does well in his presentation. Vickery himself has made important contributions to this field of endeavor, a field requiring patience, skill, and a stout heart. Separation techniques are undergoing constant improvement, and, perhaps even unbeknown to the author, it is now possible to purchase several high-purity rare-earth oxides at moderate prices; or, if one will sign enough papers, he may be favored by the loan of gram samples of the less abundant earths of an amaizingly high purity.

In an appendix, yttrium is advanced to membership in the rare-earth family, without protest or veto.

By and large the book is well and clearly written and contains numerous useful and up-to-date tables, charts, and references. If the author lapses at times into the unnecessary use of words of Latin and Greek origin, he is quickly forgiven when he presents in a straightforward and unaffected way difficult or obscure points of fact and theory. All this is highly refreshing. For we in the United States, although endowed with almost countless material blessings, show signs of regarding secrecy and evasiveness as a religion, a religion whose pagan rites are celebrated in a gold-encrusted temple furnished with an altar in which are locked samples of unmentionable isotopes; and at the altar stands, forever mute, a man who presses to his lips a document stamped in crimson with the now familiar warning. There one finds no Book, no Candle, no Bell, no Choir; only the ever mute figure at his pentagram-bedecked altar, unmoved by prayer, by reason, by errors, or even by the exasperated and raffish exclamations of another Trinculo.

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The Proteins, Vol. I, Parts A and B. Hans Neurath and Kenneth Bailey, Eds. Academic Press, New York, 1953. Vol. I, Pt. A: 548 pp., Illus., $12.; Pt. B: 567 pp., Illus., $13.

In the words of the editors, the purpose of this treatise is "to present a comprehensive, integrated account of the chemical, physical, and biological properties of the proteins." The present volume will be followed by a second one also to consist of two parts. Volume I deals with methods and with general properties of proteins as a class. Volume II will consider specific proteins, grouped according to their occurence or their functional similarity.

In recent years, the development of new tools of investigation has given tremendous impetus to research into the chemical composition and physical characteristics of the proteins. The task of providing a comprehensive account of recent progess is an imposing one. The editors have been fortunate in having the assistance of a group of 12 very able and active workers in the field. The joint efforts of the editors and contributors have produced a volume that is broad in scope and authoritative in its treatment.

In the opening chapter, J. F. Taylor presents a very comprehensive survey of methods available for the purification of proteins. The usefulness of the chapter is further increased by the inclusion of references to detailed isolation procedures for several hundred proteins. P. Desnuelle reviews the chemistry of amino acids and peptides. The description of specific group reactions is followed by an extensive discussion of hydrolysis of the peptide bond and the application of these methods to the determination of amino-acid sequences in proteins. A desirable note of caution in the interpretation of results is included. A general description, by G. R. Tristram, of methods for the determination of the amino-acid composition of proteins precedes an assessment of the specificity and accuracy of the various available techniques. Discussion follows concerning the somewhat disappointing results of studies that have attempted to correlate amino-acid composition with physical and biological properties. Barbara Low presents extensive x-ray investigations of the structure and configuration of amino acids, peptides, and proteins. Readers who are unfamiliar with x-ray techniques may not wish to follow her detailed description of crystallographic studies. They will find her general discussion of fibrous and corpuscular proteins, the section on the structure of amino acids and peptides, and the description of helical and nonhelical configurations to be of particular interest. Doty and Geiduschek, writing on the optical properties of proteins, include sections on photochemistry, ultraviolet and infrared spectroscopy, and the physical phenomena associated with nonabsorptive interactions with visible light. In a chapter on electrochemical properties, R. A. Alberty reviews the fundamental aspects of electrophoresis and of ionization of proteins and amino acids.

In volume I, Part B, J. T. Edsall examines the application of a wide variety of physical methods to the determination of the size, shape, and degree of hydration of protein molecules. Recent progress in the rapidly developing field of protein interactions is treated by Irving Klotz. Denaturation and the chemical modification of proteins are considered in chapters by F. W. Putnam. His second chapter, and R. R. Porter's chapter on chemical structure and biological activity, will particularly interest those workers who seek, in studies of protein structure, a solution to the puzzling problems of biological function.

The contributing authors have provided a broad background of fundamental theory in their fields of specialization and an up-to-date account of the scope and usefulness of current research. The applications of newly developed techniques have been stressed. Skillful editorial direction has minimized the overlap in coverage of related topics, and adequate cross references enable the reader to turn quickly to additional material in other chapters. The usefulness of the volume is greatly increased by the inclusion, in each chapter, of extensive references to the original literature. Excellent author and subject indexes at the end of Part B contribute further to the utility of this treatise.

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Synthetic Methods of Organic Chemistry: An Annual Survey, Vol. 6. W. Theilheimer. S. Karger, Basel, 1952. (U.S. distr.: Interscience, New York.) 401 pp. $12.90.

The sixth volume in the series that annually surveys new methods and improvements of known methods for the synthesis of organic compounds utilizes publications which appeared in the years 1949–50 with some references from 1951. As in the previous volumes, the author has considered largely the papers concerned with syntheses, which cannot be easily discerned from the indexes of the various abstracting journals.

Extending the system started by Weygand, the reactions are classified on a purely formal basis, such as the formation of an —O—H, S—H, N—N— bond, rearrangement, elimination and so forth. The author has thereby avoided the usage of names of reactions with which the organic chemist would be quite familiar. However, the clear description of the various symbols and abbreviations used to indicate the types of reactions enables the reader to become quickly acquainted with this system of classification.

A total of 911 abstracts appear in this volume. Most entries are accompanied by equations, followed by a short description of the synthetic method involved, the yields, and the literature source. This arrangement makes possible the rapid evaluation of a synthetic method.

It is hoped that the author will continue in this task of calling attention to the important contributions from the current literature regarding the syntheses of organic compounds.

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Principles of Polymer Chemistry. Paul J. Flory. Cornell Univ. Press, Ithaca, New York, 1953. 688 pp. Illus. $8.50.

Considerable work has been done on the chemistry of polymers in recent years, and a very extensive literature has accumulated on the subject. One welcomes, therefore, any work that serves to condense, select, and generally reorganize the thoughts and developments in this great field of study. Paul J. Flory has done an admirable job in preparing his book on *Principles of Polymer Chemistry*, a task initiated while he held the George Fisher Baker Nonresident Lectureship in Chemistry at Cornell University in 1948.

Flory effectively points out that rapid progress in polymer chemistry did not begin until the concept of

macromolecules was accepted. For a long time, polymers were regarded as aggregates held together by unidentified forces of various kinds, and the idea that a polymeric material was made up of large molecules held together by the same kinds of valence bonds that exist in ordinary low-molecular-weight compounds did not receive early recognition. As soon as it was realized that the only essential difference between polymeric and ordinary molecules is size, rapid progress was made toward understanding the preparation and behavior of polymeric systems.

After an interesting historical background followed by definitions, Flory proceeds to tell how polymers are made, either through condensation or vinyl addition. His discussion includes a thorough treatment of the kinetics of condensation, free radical and ionic polymerizations. The author then turns to the important question of structure, the determination of molecular weights, the characteristics of nonlinear polymers, and the theory of gelation. The latter part of the book is concerned with chain configurations, the thermodynamics of rubber-like elasticity and polymer solutions, and, finally, the fractional properties of macromolecules which are important for understanding solution viscosities and diffusion. The book naturally reflects those topics in which the author has been most interested. However, since Flory's own research contributions and interests have been so extensive, the coverage of the book is very broad. In this connection, it should be mentioned that there is little reference to proteins, a subject of particular importance to those leaning toward biochemistry.

The latter part of Flory's book will be especially useful to physical chemists who should welcome his excellent summaries of the theories and properties of polymer solutions; the earlier portions should be useful to organic and physical chemists alike. On the whole, this reviewer was highly pleased with the book and can recommend it without reservation to anyone interested in the principles of preparing and characterizing polymeric systems.

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The Screen Projection of Chemical Experiments. E. J. Hartung. Melbourne Univ. Press, Melbourne, Australia; Cambridge Univ. Press, New York, 1953. 291 pp. Illus. + plates. $4.75.

We have come to expect scholarly monographs from our English friends, and this volume is true to tradition. It is full of new ideas for making lecture demonstrations more visible: 5-ft test tubes, gas bubbles a foot in diameter, 3-ft cathodes. The first 100 pages discuss projection equipment; the last 200 pages describe 250 projection demonstrations. An experimental index and a general index conclude the book. Figures and plates are excellent.

I prefer a practical to the pedantic approach for the first 100 pages. For example, a half-page reference to the Polaroid Corporation kit for projection of experiments with polarized light would be better than the discussion (pp. 34–49) of this subject. Or a five-page evaluation of commercial projectors, à la Consumers Research, would help the teacher far more than the 31 pages on principles of optics in projection and the 41 pages on equipment. May the author who, in this book, has demonstrated such complete competence in his field, spend some months on a traveling fellowship in the United States, Great Britain, and Germany evaluating commercial equipment and add this material as a new chapter in the second edition.

The scope of the demonstrations in the last 200 pages is best judged by citing a few. Experiment 8, Shadow projection of gases of different densities streaming into air; 15, Water in a dilatometer contracting upon cooling to 4°C, but expanding below that; 24, C.T. and C.P. of ether; 32, Thermal expansion of wire; 41, Speed of crystallization of some organic compounds; 46, Dimorphism of red-yellow HgI_2; 51, Efflorescence of $CuSO_4 \cdot 5H_2O$; 56, Isomorphous growth of $NaNO_3$ on calcite; 69, Optical activity of crystals; 70–95, Solubility experiments; 100, Color changes with $Ag_2S_2O_3$; 107, Crystals of $K_2Cr_2O_7$ oxidizing $FeSO_4$; 114, Solid particles in a candle flame; 147–8, Formation of osmotic cells of copper ferrocyanide and of silicates; 152, Migration of ions; 156, Swelling of a Pd electrode upon absorbing hydrogen; 161, Na formed by electrolysis; 162, Formation of ammonium amalgam; 182–215, Surface phenomena; 219, Formation of colloidal gold; 235, Development of the latent image; and 246, Line spectra of metals.

This is not the death knell of the conventional lecture demonstration—small classes will still prefer most of their experiments firsthand, not projected. But this will go a long way to help the teacher in large classes, particularly Messrs. Zabrisky and Zimmerman in the back row.

HUBERT N. ALYEA

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Biology and Biophysics

Plant Growth Substances. L. J. Audus. Leonard Hill, London; Interscience, New York, 1953. 465 pp. Illus. + plates. $6.50.

This volume by Professor Audus, of Bedford College, University of London, is an important new summary of our knowledge of the chemistry and physiology of plant growth. He has undertaken to bring up to date the subject first reviewed by F. W. Went and K. V. Thimann in their classical *Phytohormones* and by P. Boysen-Jensen in *Growth Hormones in Plants* nearly 20 years ago. In the interim, our understanding of the chemical mechanisms used by plants to control and integrate their growth has increased considerably.

A development of equal importance in this 20-year period has been, however, the application of our knowledge to agriculture. The manufacture of synthetic plant growth substances has become an industry; the application of plant growth substances has become an agricultural practice; and the study of plant growth substances has spread from the academic cloister to the industrial laboratory. We know how to supervise the development of the plant by sup-

plying it with appropriate chemicals at timely intervals. In this way, we may retard or hasten the fall of leaves or fruits, we may cause the formation of new roots or buds, we may encourage or suppress the development of flowers and induce the set of fruits under conditions where fruits might otherwise be absent. We may even, if we desire, treat a mixture of plants, a plant community, in such a way as to remove species that we do not want and, therefore, call weeds without doing violence to the species that we do want, the crop.

The Audus book reflects this new direction of interest. The subject of plant growth substances is developed applicationwise. We discuss, for each application, the background of available physiological and chemical lore is given. The discussion then proceeds to matters such as methods of supplying the appropriate chemical, dosages, spreaders, duration of effects, influence of climate on the response, suitability for individual crops, and so forth. The author has, in fact, taken some pains to assure that biologist, horticulturalist, agricultural chemist, and even (he says) the gardener will each find something of interest and value in *Plant Growth Substances.*

We recognize a very considerable number of plant growth substances, each concerned with regulation of a specific aspect or aspects of plant growth (ch. 13). The bulk of our information and the bulk of this book concerns, however, just one group of hormones, the auxins. The auxin is produced, characteristically, in apical buds and other apices and is transported downward through the stem, where it is used in the control of the cell elongation process. Chapter 2 describes the basic experiments on which the auxin concept is based. The chemical identification of the native auxin of plants, a matter with which we have been concerned for more than 20 years, is discussed in Chapter 3. Audus concludes that much evidence suggests that indoleacetic acid is the native auxin of plants. The investigations of Haagen-Smit and Went in 1935 revealed that certian synthetic nonnative compounds related in structure to indoleacetic acid are able to replace the latter in its plant function. This is true, for example, of naphthalene acetic acid and of 2,4-dichlorphenoxy acetic acid, and it is upon these compounds rather than indoleacetic acid itself that agricultural applications have been based. Chapter 3 includes a full survey of the enormous amount of work that has been done in recent years on the relation of chemical structure to physiological activity of these synthetic auxins.

Although the auxins were first recognized and are still to be thought of primarily as agents that control cell enlargement, they also exert a great variety of other (and possibly secondary) effects on a variety of plant tissues. Chapters 4 through 12 are concerned with these varied responses of the plant to auxin. Effects on root and seedling growth, on rooting of cuttings, on cambial activity, on fruit development, on inhibition of lateral buds, on the herbicidal activity of auxins in high concentration, on inhibition of leaf and fruit drop, and on flowering are considered in turn. These chapters, as indicated in a foregoing paragraph, contain not only a summary of scientific background but also a wealth of detail on agricultural applicability and procedure. Brief chapters on the growth substances of roots (thiamine, pyridoxine, and niacin) and other organs, on naturally occurring plant-growth inhibitors (such as the inhibitors or seed germination contained in fruits), and on the biochemistry of growth substances in the soil complete the book.

It is a difficult matter for the author of a summary such as this book to regiment the wealth of available detail into any strict and orderly array of general principles. Audus has chosen, in general, to bring together the pertinent facts and to eschew any vigorous attempt at systematization, which is then left in a large measure to the reader himself. This is a book that will be useful to a wide range of readers just because of the wealth of matter assembled in it.

JAMES BONNER

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Catalogue of Fossil Cirripedia in the Department of Geology (British Museum), Vol. III: ***Tertiary.*** Thomas Henry Withers. British Museum (Natural History), London, 1953. 396 pp. Illus. + plates. £4 10s.

This catalog is the first comprehensive review of the fossil cirripedes since Darwin's monographs of 1851 and, in addition, is a major contribution to the study of paleozoology. This volume on the Tertiary cirripedes completes the catalog of the known fossil cirripedes with the exception of the Balanamorpha. (The Triassic and Jurassic cirripedes were dealt with in Vol. I, published in 1928, and the Cretaceous in Vol. II, published in 1935.)

The present work, which includes all the known Tertiary stalked cirripedes of the world, is an outstanding monograph. The author reexamined most of the type and figured specimens and studied many of the earlier collections as well—some 7100 specimens, of which 5650 are in the collection of the British Museum. Ninety-three species and varieties of Tertiary cirripedes, distributed among 15 genera and subgenera, are now known, as contrasted with the four Tertiary species known to Darwin. In the present volume, one new genus and 38 new species and varieties are described. In addition, the geologic and geographic ranges of both genera and species have been considerably extended and, equally important for the knowledge of this group, many species known heretofore by only one or more valves are now known by all the valves.

Terminology, ontogeny, and classification, dealt with fully in the first and second volumes, are treated only briefly in this volume. The phylogeny of the cirripedes, also covered in the previous volumes, has been amplified here. The reduction in number of valves and the change in position of the umbo owing to upward growth of the valves are fully discussed and illustrated, and some information on decalcification of the valves is added.

The chapter on the distribution of the Tertiary cirripedes includes a general discussion of the geologic formations, a list of species arranged geographically, a stratigraphic survey of marine Tertiary beds, and a review of the fauna. The liberal use of tables and summaries in this chapter makes the comprehen-

sive information more readily available to those interested in the subject.

The largest chapter in the book (pp. 99–352) is that covering the systematic descriptions of the suborders Lepadomorpha and Brachylepadomorpha. In addition to a concise diagnosis of each family, genus, subgenus, and species of Tertiary cirripedes, systematic discussions of each genus and many species are included. Here is correlated all the previously published information on these genera or species with new information elucidating their geologic and geographic relationships. The illustrations for each species add to the value of this chapter as well as to the concluding chapter on the noncirripede or doubtful species.

A complete bibliography and well arranged index complete the volume. The author is to be highly commended for this excellent contribution to our knowledge of barnacles.

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Primates: Comparative Anatomy and Taxonomy, Vol. 1: ***Strepsirhini.*** W. C. Osman Hill. University Press, Edinburgh; Interscience, New York, 1953. 798 pp. Illus. + plates. $12.50.

The primates have attracted more attention than any other group of animals, and in consequence the literature dealing with them—excellent, competent, and bad—has reached enormous proportions. The last good general summary of the order is 60 years old, and the only attempt at a partial synthesis published in the interim, that by Elliott in 1913, is almost as much a hindrance as a help. A situation such as this is quite enough to discourage almost anyone from undertaking a general compendium, and Dr. Hill must be congratulated on his courage in making the attempt. He has strived to give a broad survey. Although the subtitle reads *Comparative Anatomy and Taxonomy,* the treatment also extends to what is known about development and behavior. Distribution in time, as well as in space, is considered, a generous amount of text being devoted to extinct forms.

Following Pocock, Hill divides the primates into two "grades," the Strepsirhini and the Haplorhini. The former, the subject of this volume, comprises the lemuroids, including the extinct European and North American families usually grouped with them, and the lorisoids, both regarded as suborders. His treatment of this assemblage is prefaced by a short general discussion of the anatomy, reproduction and development, distribution, taxonomy and phylogeny of the order as a whole. This is followed by a more extended summary of these features in strepsirhines. The same general plan is adopted for the suborders, families, genera, and even species, which inevitably entails a certain amount of repetition. Subspecies are treated quite fully, but in small type, "so as to avoid embarrassment to the seeker after purely general or morphological information."

A number of keys are provided, and there are many attractively executed distribution maps. The majority of the plates illustrate mounted skeletons and captive specimens or mounted skins, and many of the figures are devoted to external characters, particularly the genitalia. The index is adequate, a very important feature in a work of this nature. The date of publication is 1953, but it is evident from the bibliography that the manuscript must have been completed early in 1948, a point that users of the volume should keep in mind.

It is open to question whether Pocock's classification of the primates is the best of several alternatives. It is true that in other orders the presence or absence of a rhinarium, the key character separating strepsirhines and haplorhines, is remarkably constant, but it still does not necessarily follow that the earliest tarsioids lacked one. The various other features that *Tarsius* has in common with higher primates, which occur in a matrix, so to speak, of lower primate characters, may well have been independently acquired. In fact, to the reviewer at least, it would seem that the old threefold division into Prosimii (including tarsioids), Platyrrhini, and Catarrhini accords rather better with what is known of the history of the order. Hill excludes the tupaioids from the primates, a step for which a good case can be made, and does not discuss them at all. This omission is a pity, because regardless of the taxonomic position assigned them, in part merely a matter of definition, there is little or no room for doubt that of all living mammals they are the closest to the primates. As such, some consideration of them is highly desirable in any comprehensive work devoted to the order. Their inclusion would not have greatly increased the magnitude of the formidable task the author set himself.

Discrepancies occur in various parts of the work. For example, on page 11 it is reported that no certainty exists on whether the metadiscoidal or the deciduate and hemichorial type of placentation is primitive for the order, whereas page 92 informs us that conditions in *Galagoides* settle this question in favor of the metadiscoidal. Numerous characters are given as diagnostic of lorisoids as opposed to lemuroids (p. 105), but later on (p. 318 *ff.*) we learn that a number of these are also present in the cheirogaleine lemuroids. It is unreasonable to expect the author to be equally well versed in all the fields he has attempted to cover, but some errors or misinterpretations of fact in paleontology, the field that may be least familiar to most of those who will use the volume, must be pointed out. Eocene primates are not unknown in Asia (p. 19); they are present in the Late Eocene of Burma. The table showing Tertiary chronology (p. 21) is long out of date with regard to North America. It is true that primates do not appear in Europe until the Late Paleocene and "even then they are represented solely by plesiadapids (p. 19)," but this does not mean that a varied fauna did not exist there during that time. The facts are that in Europe no Early and Middle Paleocene mammalian faunas are known, and knowledge of Late Paleocene mammals is based essentially on material from a single horizon. If our knowledge of the Paleocene in North America were similarly restricted, it is quite likely that only *Plesiadapis* would be known here too.

The more ground covered by a work, the more, of course, a reviewer can find to carp about. The important question is always: how useful will the work be? To this it may be confidently replied that the situation with Hill handy on the shelf is infinitely better than before, and this is surely praise enough.

BRYAN PATTERSON

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The Polyporaceae of the United States, Alaska and Canada. Lee Oras Overholts; prepared for publication by Josiah L. Lowe. Univ. Michigan Press, Ann Arbor; Oxford Univ. Press, London, 1953. 466 pp. + 132 plates. $7.50.

Those who knew the author will not be surprised at the substantial excellence of his achievement. About 40 years ago, he familiarized himself with polypore anatomy and basic techniques. Twenty years ago, the author published diagnostic keys which, being critically used, were perfected for this book.

In his introduction, Overholts recognizes basic work of such Americans as Lloyd, Murrill, Schweinitz, Peck, and others, and Europeans including Patouillard and Romell. The author concludes this chapter with a discussion of the early history of the Polyporaceae, generic segregates, morphology, anatomy of the hymenium, economic importance of the family, wood decay, his concept of parasitism as it pertians to tree-invading polypores, and pure cultures in taxonomy. Finally, the author presents a taxonomic account of all known species of pileate Polyporaceae of North America north of Mexico and southern Florida. Most students will be pleased that Overholts has held to his conservative generic concepts.

Of the 466 pages of text, some 75 pages are devoted to keys, notes, and descriptions of 40 species of the genus Fomes; 266 pages are given to 162 species of Polyporus. In all, eight genera are appropriately treated.

Nomenclature employed follows the International Code. Technical descriptions, based on examination of numberless specimens, are well drawn and, so far as checked, bear a practical relationship to the diagnostic key. Habitat notes are also based on the examination of many specimens collected widely over the continent. Geographic distribution is presented through a listing of the names of states from which the author has examined collections. Carefully selected illustrations published by others are cited. The text is followed by a bibliography of 238 titles, valuable in that it lists important European, as well as American, publications.

Of the illustrative plates 124 are halftone plates bearing 675 figures. Gross structural details are clear, and it appears that good judgment was exercised in the selection of the photographs. Five plates carry line drawings prepared to show the chief characters of the hymenium for scores of species. The volume closes with an adequate glossary and index.

As many of Dr. Overholts' friends well know, he passed away in 1946, before the manuscript was completed. It was then that Dr. Frank D. Kern, a colleague of Overholts, prevailed on Josiah L. Lowe, of Syracuse University, to assume the burden of preparing the manuscript for publication. Little imagination is required to appreciate the complexity of such a task.

For reasons of health, Overholts had not incorporated some 30 species and varieties that have been described since 1938, most of them by W. A. Murrill. Dr. Lowe, in preparing the final manuscript, included such species in their appropriate places as Omitted Species. In completing this task, Dr. Lowe proved himself worthy of a difficult assignment and gained the everlasting gratitude of science. Finally, the University of Michigan Press in publishing the manual has maintained its usual high standard. The volume fills a need, and does it superbly and with authority.

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Phylogeny and Morphogenesis: Contemporary Aspects of Botanical Science. C. W. Wardlaw. Macmillan, London; St Martin's Press, New York, 1952. 536 pp. Illus. $7.50.

It is somewhat more than a year since Wardlaw's *Phylogeny and Morphogenesis* appeared. A few months earlier, Methuen had published his shorter discussion *Morphogenesis in Plants.* The early account described representative experiments carried out by the author on apical meristems of the shoot systems, especially of ferns, and discussed the implications of these experiments in the interpretation of the genesis of form and structure.

In *Phylogeny,* the author has described and interpreted many of his experiments more fully. He has also emphasized the importance, and indeed the indispensability, of developmental studies to the phylogenetic worker. The paleobotanist may well avoid errors by knowing the variables of ontology and the possible impact of environmental realities such as nutrition, light, and moisture, upon the extent of genetic variation during development. Since phylogenetic trends proceed as a result of the survival of organisms with genetic modifications expressed during development, then certainly the two seemingly separate disciplines of phylogeny and morphogenesis must be directly connected and can with justice be considered in a single volume. Wardlaw states it thus, "Phylogeny and morphogenesis are not separate disciplines: they are one . . . each being essential to the other." Certainly Wardlaw's own training in comparative morphology under F. O. Bower, at Glasgow, has borne fruit in the morphogenetic findings of his recent fascinating experimental studies.

Of the 20 vigorously written chapters, and bibliography of approximately 1000 titles, comprising *Phylogeny and Morphogenesis,* seven chapters (125 pp.) consider the phylogenetic point of view, historical and present. The remaining 351 pages (13 chs.) represent a significant assessment of the whole subject of morphogenesis in vascular plants. The book is not only an epitome of extensive studies from Wardlaw's laboratory at Manchester; it comprises one or more chapters each on genetical, biochemical, physical, and mathematical aspects of morphogenesis, with appraisal of pertinent investigations in these fields. For the student of morphogenesis of the higher plants, much fundamental spadework has been done in this assemblage of cogent material from diverse approaches.

For the paleobotanist and general student of comparative plant morphology, Wardlaw points out that although experimental investigations are necessarily based on living plants, "a clearer understanding of the factors that determine form and structure in living plants" should permit "a more adequate interpreta-

tion of the developments indicated by the fossil record." The earnest botanist looking for new leads may find himself excited by the possibilities of utilizing the microscalpel and the culture tube to supplement the microtome and the microscopic slide. He may even be drawn to explore the experimental advantages of some vascular cryptogam instead of, or as a preliminary step to, an angiosperm for experimentation.

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Physiological Acoustics. Ernest Glen Wever and Merle Lawrence. Princeton Univ. Press, Princeton, N.J., 1954. 454 pp. Illus. + plates. $10.

Physiological acoustics is defined in this book as the events that transpire between the impingement of sounds upon the ear and the arousal of nerve impulses by them. The text, therefore, deals with detailed measurements of the mechanical excursions of the eardrum, middle-ear bones, and cochlear contents. It will be a surprise for many to discover how many such measurements exist and a real help, for those sophisticated enough to know of their existence, to find them systematically assembled in a single place.

The authors, however, do not deal merely with a body of facts; theories derived from or supported by them weave in and out of the text and serve to bind what might be a set of tables into what can be called a book. These theories stem from many sources, which means that there are many of them. The authors describe each theory in simple declarative sentences and evaluate most of the theories, using either the available published experimental evidence or experiments reported here for the first time. The end-result is not easy reading, nor will it appeal to a general audience; the experts, however, will find much here both to agree and disagree with.

Toward the end of the book, some 50 pages are devoted to otosclerosis, a type of deafness in which violation of the normal laws of physiological acoustics occurs owing to fixation of the footplate of the stapes. In this section, what is known about these laws is brought to bear upon the practical clinical problem, and the discussion of the fenestration operation that often relieves the deafness serves to focus the fact and theory in an interesting and informative way. This section will have the most general appeal, and in some ways it is the best of the book.

In the final section, there is a brief account of the neural mechanism of perception of pitch. As the authors see it, the two familiar concepts, the place and volley principles, operate in the way Professor Wever discussed in his recent book.

A glossary of terms, an adequate index, and five appendices—some very useful—round out this book. The list of 383 references, while not exhaustive except for the work of some authors, covers the field completely.

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Nerve Impulse. David Nachmansohn, Ed. Josiah Macy, Jr. Fdn., New York, 1954. 224 pp. Illus. $4.

This fourth conference on the nerve impulse had for its program the following topics: Mechanism of vision (Wald), Mechanism of Hearing (Davis), and Sensory Receptors (Zotterman).

In the presentation of the mechanism of vision, the facts are most ably set forth, but neither the paper itself nor the discussion develops anything that can be construed as a connection between the initial photochemical event and the subsequent nervous excitation. Hartline notes that at threshold the delay between light absorption and excitation may be one second, and that certainly the photochemical excitation cannot last this long. The discussion turns to a consideration of how the bleaching of one molecule of rhodopsin can be amplified into some sort of biochemical change; it is not at all clear where the free energy to operate this amplifier is to come from.

The mechanism of hearing now appears to involve many new factors not dealt with in conventional neurophysiology. Some of the more remarkable of these are the marked differences in Na^+ concentration between labyrinthic fluids and direct current with superimposed alternating-current potentials in the labyrinth. It is apparent from the presentation that the subject is in a rapid state of development, but the discussion of these results by the group is rather disappointing. Communication difficulties appear; the participants divide into the electro-analog and the biochemical groups; and no middle ground for discussion appears. The fault may lie in the very newness of the data, and the fact that there has been little time to develop coherent schemes for changing sound into nerve impulses within the framework of this new work.

The treatment of sensory receptors concerns mainly the activity of the thermal receptors in the cat's tongue. Again there are conflicts with conventional notions of the way stimuli produce changes in receptors. The cold receptor has an electric activity dependent upon temperature as well as an activity dependent upon changes in temperature; it is the first finding that is unusual—that a constant stimulus produces a continuous response. The presentation and discussion are a valuable summary of recent work on thermoreceptors. As might be anticipated, considerable difficulty arises when explanations are sought for the reason thermal sensitivity of a small fraction of a degree centigrade can exist in a receptor. An entirely personal feeling of the reviewer is that authors and participants ought to be allowed to delete from these printed discussions some of the more unfortunate remarks that are made.

L. J. MULLINS
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Textbook of Genetics. William Hovanitz. Elsevier, Houston–Amsterdam, 1953. 419 pp. Illus. $5.95.

This new textbook of genetics is distinguished from other books in the field by an extensive treatment of elementary or "formal," genetics and of the statistical techniques used in this field. Of the more advanced topics of genetics, population genetics is emphasized at the expense of physiological and developmental genetics.

Elementary genetics, including linkage and the mapping

of chromosomes, accounts for 10 of the 18 chapters. The chromosome theory of heredity is introduced as a statement on page 3, and the distribution of genes in crosses is derived from the behavior of the chromosomes at meiosis. This procedure may be deplored by teachers who prefer the historical approach, particularly since it involves omission of some of the elegant experiments which form the evidence for the chromosome theory of heredity. The present way of presentation is, however, didactically just as sound and may be easier to understand in an introductory course. In the examples given, *Drosophila* and corn genetics are more thoroughly treated than in most elementary textbooks. The statistical methods are introduced in appropriate places in the text and are thoroughly and competently explained.

Of more advanced topics, chromosome rearrangements and changes in chromosome number are well covered. The chapters on selection and populations and on evolution are very clear and concise. The chapter on mutations does not consider the more recent developments in this field. There is only one chapter on biochemical and developmental genetics that includes the discussion of sex determination and of cytoplasmic inheritance. Bacterial genetics and the genetics of *Paramecium* are barely mentioned. One chapter deals with human and agricultural genetics.

The style of the book is occasionally awkward, and although the author's meaning is always clear to a trained biologist, some statements may be bewildering to beginning students.

At the end of each chapter, there are a number of well-conceived problems and a bibliography. There is a large number of very good illustrations, many of them original. Two appendices contain a direction for laboratory experiments with *Drosophila* and corn and an abbreviated version of Warwick's well-known tables of Mendelian ratios in small numbers.

It may be expected that this will be a useful textbook for introductory courses in genetics.

ERNST CASPARI

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The Anatomy of the Migratory Locust. F. O. Albrecht. Athlone Press, University of London, 1953. (U.S. distr.: Curtis Brown, 347 Madison Ave., New York 17.) 118 pp. Illus. 30s or approx. $4.25.

This book is a concisely written, well-illustrated manual of much value in academic laboratories where grasshoppers are being used as subject animals in teaching, and it is also a basic guide for specialists in acridology. Undoubtedly this manual will give impetus to the increased use of grasshoppers as experimental animals, since they have been found to be excellent representatives of the arthropod phylum, while at the same time laboratory studies provide basic information of value in applied studies. The author conducted his work at the University of London, and he is now a staff member of the Anti-Locust Research Centre.

Here it may be helpful to explain to American biologists that the distinction between grasshopper and locust is not a sharp one. In essence, locusts are grasshoppers that tend to be outstandingly migratory and gregarious in behavior, and they have anatomical features which usually are correlated with greater flight capacity.

The title correctly gives the scope of this work as anatomy, since no attempt at comparative morphology with other Acrididae is made, and little attention is given to function. The style is brief and to the point, and the various parts of the body, first externally, then internally, are reviewed in order, illustrated with clear line drawings by the author. The appendix of instructions for dissection will be very helpful.

The author states that the Snodgrass series of papers on grasshopper anatomy has been of the utmost value, and to a large extent he has been guided by Snodgrass in interpretations and choice of terms. Descriptions are brief, and certain details of specialized parts of the body, such as the patches of sensory hairs near the vertex of the head, described in recent years by Weis-Fogh as aerodynamic sense organs, are not mentioned. A list of 34 references is included. Evidently, here again the aim is to give the student what is most important and not to clutter his mind and time with details that he will discover later if he continues the study of grasshoppers. Papers by Snodgrass, Slifer, E. M. Walker, and Vinal are among the basic American works cited.

Acridid specialists may not be accustomed to certain of Albrecht's terms. He apparently prefers *protergum* (which is correct morphologically) to the more familiar *pronotum*, and he uses *spines* instead of *spurs* for the movable structures at the apex of the tibiae. Wing veins are named according to a modified Comstock-Needham system, and the interpretations are said to be based on recent unpublished studies by D. R. Ragge, of London. Although many British students of acridids apply *elytron* to the front wing, Albrecht calls it the *tegmen* as is usually done in America.

It may be predicted that this book will rank as a fundamental contribution, especially because it covers all body systems. The author is logical and has done will in his attempt to provide a manual that will help other students.

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General Virology. S. E. Luria. Wiley, New York; Chapman & Hall, London, 1953. 427 pp. Illus. $8.50.

This textbook offers for the first time a remarkable synthesis of the facts and concepts of a new science that is now emerging from numerous scattered contributions of many workers, often having different purpose and interest. This new science studies "the virus" from a chemical and physical point as an inert particle and as an operating constituent of functional cells. In this book, information from the field of bacterial, plant, and animal viruses is pooled together, although the important differences among them are duly emphasized.

Because of the author's background as a phage worker, one might have expected the book to be essentially devoted to bacterial viruses. This is not so, for equal consideration is given to each branch of virology, and none of its significant aspects has been neglected.

The main characteristics of this book are the excel-

lent definition of the problems, the accuracy and extent of the information, and the critical evaluation of the data. The experimental results presented are numerous (697 references) and amazingly up to date; many neat and easily understandable tables are included, as are also effective illustrations.

The first seven chapters of the book discuss the general properties and physical and chemical characteristics of the viruses. The statistical and physico-chemical principles involved in the determination of some characteristics are adequately discussed. The concept of purity of virus preparations is discussed in an acute and original way. The following five chapters discuss the interaction of viruses and of their hosts in bacteria, plants and animals. Other chapters discuss hemagglutination phenomena, interference, and variation of viruses. After discussing the transmission of viruses and the tumor problem, the book ends with a general discussion of the origin and nature of viruses. A discussion of the Rickettsiae is given as an appendix.

The book stresses the problems of greatest actuality: for example, one chapter is mainly devoted to the use of tissue cultures in animal virology; another, and an inspiring one, to the relationship between viruses and tumors. In discussing debated questions, which are numerous in a new science, the author restrains from taking too sharp a position, although his attitude is always critical and progressive. The practical implications of many biological properties of viruses are not neglected, as proved by the extensive discussion of the medical aspects of virus serology (ch. 6), of the problems of transmission of plant and animal viruses and of their epidemiological significance (ch. 16), and of many other points.

Owing to the considerable number of facts and to the high level of the discussion, the book, in a few places, may not be easy to read; mature students will profit most from it.

All together the book is truly outstanding. It will constitute exciting reading for everyone interested in some of the most intriguing aspects of biology.

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The Medusae of the British Isles: Anthomedusae, Leptomedusae, Limnomedusae, Trachymedusae, and Narcomedusae. E. T. Browne Monograph of the Marine Biological Association of the United Kingdom. Frederick Stratten Russell. Cambridge Univ. Press, New York, 1953. 530 pp. Illus. + 35 plates. $22.50.

Every devoted systematist must secretly cherish the hope that he might emulate the example of Alfred Goldsborough Mayer who wrote, in his *Medusae of the World,* "I have always felt that each working naturalist owes it as a duty to science to produce some general systematic work," but few have been able to do so in these days when so many things seem more important than a knowledge of the species of animals. Yet, without such knowledge, much practical work in oceanography, agriculture, physiology, and medicine would be impossible. It may well be true that Dr. Russell's original motivation for taking up the study of medusae is their utility in studying water movements, but it is plain that he is a zoologist and a devoted systematist as well, for it took perseverance and interest to carry this work on to its delayed but successful conclusion.

All the species known to occur in British waters (some 90 species, including the one fresh-water form) are discussed, and a number of possible species, not surely known for the area, are also considered. Exhaustive synonymies are given for most of the species, although the author has had to throw up his hands once: "Sufficient has already been said to indicate the confusion that exists in the synonymy of *Obelia.* There is nothing to be gained by further mention." Nevertheless, the highest traditions of scholarship have been met, and every effort has been made to make this work useful. Many of the drawings are of preserved material, rather than idealized perfections seldom seen in collections, and a neat pictorial key consisting of 80 vignettes is offered in place of the usual verbal dichotomy. The bibliography on *Craspedacusta* is listed separately from that on the marine species. Citations are not restricted to regional references.

From the standpoint of zoology, it is of interest to note that the author makes no attempt to harmonize the divergent classification of hydroid and medusae stages, since he is of the view that our knowledge of the hydroids is still too imperfect. Rather limited use has been made of nematocysts, and it may well be that this is the last major monograph that does not at least make the attempt to ascertain the validity of this character in a comprehensive manner, although much remains to be done before we can decide whether this character may be used for both hydroid and medusoid stages.

Publication of this work on such a handsome scale has been made possible in part through the bequest of E. T. Browne, himself a student of medusae and in a sense the inspirer of this work. It is a splendid piece of bookmaking, and the color plates look fresher and more lifelike than those in Mayer's monograph.

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The Medical Sciences

Adventures in Physiology: A Selection of Scientific Papers. With excursions into autopharmacology. Henry Hallett Dale. Pergamon Press, London, 1953. (U.S. distrib.: Macmillan, New York.) 652 pp. Illus. $19.50.

This remarkable volume presents a dramatic portrait of one of the greatest and most beloved scientists of this century. It is principally a reproduction of his important papers with comments by the author which bring the facts and ideas of the past into relationship with the current state of knowledge. The papers chosen by Sir Henry are concerned with his two main areas of research and serve to emphasize his extraordinary versatility as a scientific investigator. In their pursuit he utilized the techniques and background of several scientific disciplines including pharmacology, physiology, and experimental pathology. In the author's own words:

These two lines of inquiry have led, on the one hand by way of studies which involved the specific actions of adrenaline and acetylcholine, to a widening application of the conception of a chemical phase in the transmission of excitation from nerve fibre endings to responsive cells; and, on the other hand, by way of studies of the actions of histamine and of its distribution in the animal body, to evidence for its contribution to local and general reactions, by which the organism as a whole and its separate tissues respond to various chemical, immunological or physical assaults upon the integrity of their living cells.

The introduction, written by the author, gives a biographical sketch of his scientific career. He relates the circumstances surrounding many of the scientific discoveries that are described in the body of the volume. The result is a fascinating story which has many points of interest and lessons for the physician, the teacher, and the investigator whether he is in a university, a commercial, or a government laboratory.

During the early part of his career, Sir Henry received guidance and inspiration from many eminent members of the great school of British physiologists, including Gaskell, Langley, Anderson, Bayliss, and Starling. In 1904, he accepted a post in the Wellcome Physiological Research Laboratories against the advice of many friends. Here began one of the most productive scientific careers of our era and also one that provides an important illustration of serendipity in medical research. It was suggested that he attempt to bring some clarity into the problem of ergot—the knowledge of its pharmacology being then in a state of confusion. This project given to a young scientist, to whom pharmacological research was a novelty, presented no attraction for him. However, out of the work, which eventually led to a Nobel laureate, arose both of his main themes in research.

On several occasions, the keenness of his observations and the awareness of their possible importance led him to make important discoveries that had no primary relationship to his basic assignment.

His first good fortune came from making what he labels a shocking "howler." While finishing an experiment concerning the blood pressure effects of an ergot extract on the spinal cat, he was asked to test a preparation of dried suprarenal gland substance fresh from the Wellcome factory. Successive injections gave only falls in pressure and with the "confidence of inexperience" he condemned the sample without hesitation. Then, by one of those coincidences which make life so remarkable, the same sequence of events occurred the following week. On referring to his notes, he quickly saw the correct answer; thus, not only was the reputation of the young pharmacologist saved in the eyes of the company, but the basic discovery of the effect of ergotoxine on the pressor response to adrenaline was made.

Later, examination of the products formed by putrefaction in the preparation of the official "Liquid Extract of Ergot" led to the study of a variety of pressor amines and the coining of the term *sympathomimetic,* researches which have been of great general importance in medicine. Also, after seeing Kehrer's demonstration of the tonic effect of an ergot extract on the cat's uterus, he recognized that this must be caused by an active substance of a different kind from any he had previously studied. This led, in collaboration with Barger, to the isolation of histamine, and many studies of fundamental value followed which form the first important group of papers reproduced in this volume.

In 1913, another accidental observation he made, while testing an ordinary liquid extract of ergot, led to the detection of acetylcholine and stimulated his interest in what was the most exciting venture of his life: his studies concerning the chemical transmission of excitation at the junctional contacts between nerve endings and cells. Fifteen years later, while engaged in a series of studies on the distribution of histamine in normal tissues and organs. Dale and Dudley quite unexpectedly found acetylcholine in the spleens of oxen and horses, thus being the first to show that this substance is a natural constituent of animal tissue.

As the author points out, when one reviews this succession of events, it is clearly evident that accidents frequently provided new directions to his research, Most of these "accidents" stemmed from investigations into the problem originally assigned to him by Mr. Wellcome and led him along fruitful paths quite unrelated to it. What the author does not point out in his extraordinary ability which allowed him to perceive the meaning behind each event in this "succession of accidents." This volume not only reproduces a selected series of scientific papers of importance presented in a most interesting way, but also the story of this successful scientific career unfolded in autobiographical style is an exciting and valuable document in its own right.

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The Conquest of Plague. A Study of the Evolution of Epidemiology. L. Fabian Hirst. Oxford Univ. Press, New York, 1953. 478 pp. Illus. + plates. $11.00.

This fascinating and highly informative volume is concerned with the natural history of one of the world's most deadly diseases, the bionomics of its natural host, the rodent, and the impact of the infection on primitive, medieval, and modern man. Beginning with the earliest records of plague in ancient Babylon and continuing through its many devastating epidemics in Asia, Europe, and North Africa, the conception of the origin of the pestilence passed through one period after another of animistic superstition, then miasma and contagion, until, during the last decade of the 19th century and the first decade of the 20th century, the etiologic agent and the methods of its transmission were discovered. It is repeatedly emphasized that urban plague is primarily a disease of the semidomestic Norway rat, and that the most important vector is the tropical rat flea, *Xenopsylla cheopis,* which spreads the plague bacillus to

other rats, including the domestic black rat and its relatives, and through these rats to man.

During the last pandemic, which originated in Hongkong in 1894, the disease was disseminated for the first time to the Western Hemisphere and to Australia. Here, as in Asia and Africa, it became established in extensive areas in native wild rodents. Although these enzootic foci constitute a continuing potential for extension of plague to human communities, domestic rodent plague is greatly reduced and human exposure is much less than it was previously. This conquest in rodent plague has resulted from an understanding of the ecology of the reservoir hosts and their flea ectoparasites, the climatic factors which favor rat breeding and flea densities, the transmission of the plague bacillus, and the institution of measures to prevent the breeding of urban rats and their parasites. Furthermore, modern chemotherapeutics when administered early in the disease provide a favorable prognosis, and immunization offers considerable protection for the exposed human population. Thus the conquest of plague, like that of malaria, yellow fever, and typhus fever, has been achieved by discovery of the agent and its arthropod transmission and by the institution of measures to interrupt the cycle.

The author is well equipped by extensive experience in plague areas of Asia to write authoritatively on the subject from a modern viewpoint. The historical background has been developed by an exhaustive critical study of the literature. The work is extensively documented and is illustrated by a few well-selected photographs, charts, maps, and graphs. The style is essentially narrative and is unusually lucid for a scientific publication. Good reference lists to the literature are found at the end of each chapter. There are separate author and subject indexes. The format is pleasing, and no typographical errors have been found.

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Problems in the Anatomy of the Pelvis: An Atlas. Eduard Uhlenhuth, with assistance of DeWitt T. Hunter; illus. by William E. Loechel. Lippincott, Philadelphia–London, 1953. 206 pp. Illus. $10.

Professor Uhlenhuth's monograph is an atlas prepared for the use of those engaged in urological and gynecological surgery and for the guidance of teachers who are called upon to instruct postgraduate students or hospital staff members in either of these specialties.

Influenced by a defensible surgical bias, consideration of the anatomy of the retrovesical space is given first place, its description being Part I of the book. This space is made the hub of the discussion, upon which the accounts of all other subperitoneal areas and their organs are, so to speak, convergent. Parts II and III then contain specialized descriptions of the musculature of the urinary bladder, rectum, and pelvic diaphragm. The text, with a selective list of references, covers the first 66 pages; the figures, their legends, and two indexes use the remainder of the volume.

Among the important anatomical structures discussed in the text are the following: the vesical and prostatic capsules; the ductus deferens and seminal vesicles; the trigone of the bladder; the urinary sphincters (the structure of which has been the subject of sober, long-term controversy); the levator musculature, its divisions and variably lamellar character.

The illustrations were prepared either from original dissections or from roentgenograms of cadavers. In the preparation of the former, the author was assisted by De Witt T. Hunter; in the execution of the figures, he is indebted to William E. Loechel, his departmental medical artist. The dissections are ingenious; the drawings are clear, the reproductions first class.

This product of painstaking laboratory study offers to the inquisitive student of pelvic structure very substantial material for self-improvement either as a surgeon or as an anatomist. Its content is not readily mastered—but never could be made easy of acquisition, simply because of the inherently difficult nature of pelvic layering.

The use of numbered items accompanied by explanatory key is somewhat unfortunate; labels, placed on structures or employed with leaders, would have rendered study more expeditious. Some new terms are coined, special to the portrayals in the figures.

Altogether, the treatise offers its own tribute to Uhlenhuth's acumen and persistence. The author's work is a refutation of the lazy conclusion, complacently reached by some anatomists, that, since all fasciae are fibrous, segregaiton of laminae and consequent production of spaces are procedures both arbitrary and unrewarding.

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Symposium on Fatigue. W. F. Floyd and A. T. Welford, Eds. H. K. Lewis, London, 1953. (For the Ergonomics Research Society.) 196 pp. Illus. + plates. 24s.

The symposium unites a number of recent contributions to the many-sided problem of human inadequacy generally called fatigue. Of the 20 contributors to the symposium held in England in 1952, four are Americans, namely Ryan, Schwab, Kennedy, and Gagné. All told, a great variety of aspects of the work and inadequacy problem were presented either experimentally or by reference, but in no case was a general comprehensive, systematic treatment of the problem given.

Those who gave most attention to nonexertional factors were the two participants reporting upon "tropical fatigue," a syndrome that arises without sufficient physical changes to account for the inefficiency. Even here, the inefficiency rather than the identifiable experience of the victim and its relation to the task or situational demand is the focal point in the first participant's thinking. In the second, tropical fatigue is pointed out as sometimes being tropical neurasthenia. In this paper such broad factors as whether or not the individual wants to live in the tropics are thought to be significant.

Bartlett made the most comprehensive definition of fatigue in the symposium. His definition made the term

fatigue cover so much that other terms were required to designate the specific forms of change that occur in the organism, as, for example, impairment for the intracellular, and some other word for the experiential.

One paper by Browne was "Fatigue, Fact or Fiction?" He said that what most workers mean by fatigue is simply "decreasing performance," therefore, why not call it that, and that any single entity called fatigue is a fiction. Muscio reached the same conclusion many years ago. This sidesteps the existence of the identifiable self-experienced state that people in general call fatigue and that expresses the relation of a *person* to a task.

The failure of research workers, even as yet, to attempt systematic treatment of the problem of human inadequacy is a more important thing to point out here than factual contributions made by the participants. This failure must represent some sort of implicit attitude toward the general problem—probably the belief that systematic formulations would be premature, or that we can always get along well without them. On the contrary, if systematization is still premature with all the myriad facts we have, it always will be. Thus we should always be guilty of the same muddled thinking and overlapping terminology we have at present, despite the ingenuity of workers to investigate, and the ever-increasing accumulation of their findings.

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The Physiopathology of Cancer. Freddy Homburger and William H. Fishman, Eds. Hoeber-Harper, New York, 1953. 1031 pp. Illus. + index. $18.

At this time when the study of cancer needs coordination of information and interdisciplinary approaches as well as further specialization, Homberger and Fishman have attempted to assemble in one volume the present-day knowledge in all active fields of cancer research. As could have been expected, the result is a tome that is much too heavy to be carried around and read at one's leisure, but it is one that should occupy a prominent place on the desk of the investigator and the physician and one that will be of use to the student preparing himself for research in cancer.

Apparently realizing that the scope of cancer research can no longer be covered adequately by a single author, the editors have followed the rather frequent practice today of inviting outstanding investigators to contribute chapters in their respective fields of specialization. Probably the editors' greatest success with this book was in their selection of the 28 contributing authors.

Just as one author could not be expected to write a book adequately covering all fields of cancer, neither should one individual be so presumptuous as to attempt a detailed appraisal of all chapters in this volume. After having read the book, however, one can give a general description of it and present certain impressions he has gained with detailed evaluation limited to certain chapters.

The book has been divided into four sections: Biology, Chemistry and Physics, Clinical Investigation, and Practical Applications. The first two parts, written by investigators in the more basic fields of research, appear to be written for the investigator. Under Biology are presented discussions on morphology of tumors, carcinogenesis, genetics, endocrinology, virology, nutrition, and transplantation of tumors. The second section has two chapters linking biology and chemistry in the mechanisms of carcinogenesis, with others presenting discussions of the chemistry of cancer including nucleoproteins, enzymes, cytochemistry, and histochemistry, and also a discussion of experimental chemotherapy and radiation. The third and fourth sections are directed toward the clinical investigator and the physician. The third section gives a general discussion of clinical investigations in cancer with special chapters on steroid metabolism and problems in occupational cancer and statistical studies. The fourth section includes an evaluation of diagnostic tests, a discussion of diagnosis by the use of exfoliative cytology and of isotopes, and chemotherapy and radiation therapy.

Throughout the volume, standards of presentation are high, although there is, of course, great variation in manner of presentation and type of material given. Two extremes are represented by the chapter on genetics and the chapter on viruses. The former is a faithfully recorded chronological account of the work published in this field with very little deviation from the conclusions of the authors. In contrast, in the chapter on viruses, the author is vigorously selling an idea. Although many of his readers will not be convinced that all types of cancers are caused by viruses, all will agree that he has presented a very stimulating discussion. Many of the chapters, such as the one on nutrition and the one on endocrinology, contain a vast amount of facts that have been firmly established, whereas others, such as the two on chemical and genetic mechanisms, contain discussions one might not expect to find in a book of this type, since some of the facts are not so well established. These two chapters are thought provoking nevertheless.

The extensive experience of the authors of the chapters on morphology is clearly evident. Future investigators will be fortunate to have these published observations. This section, however, is not well rounded, for, whereas there is a whole chapter on testicular tumors and another of endometrial carcinoma in the rabbit, both of which are of more limited use and interest, there is no chapter on leukemia or any of the other neoplasms of the blood-forming organs. Morphology of neoplasms in man already amply dealt with in many textbooks is not included. The photomicrographs in this section and elsewhere are excellent. There are not as many, however, as might be desired. Some of the chapters describing the morphology of specific groups of tumors have none. From her vast number of observations of mammary gland tumors, the author of this chapter has presented a classification that undoubtedly will receive wide acceptance. Unfortunately, errors have occurred in printing the legends for some of the figures. (The legend for Fig. 37 should have been that for Fig. 38; the legend for Fig. 38 should have been that for Fig. 39; and the legend for Fig. 39 should have been that for Fig. 37.)

Present knowledge on transplantable tumors covering both the genetic and the immunologic aspects is well summarized. This section should be of great interest to the plastic surgeon as well as to the cancer investigator. To one who has read the chapter carefully, the editors' footnote which implies that from the work of transplant-

ing human neoplasms to experimental animals by the use of cortisone or various techniques we are going to have to modify our concepts of these basic principles of tissue transplantation, hardly seems justified.

As a whole, the problem of cancer is presented as one in biology. Nevertheless the chemistry subjects are adequately presented. Furthermore, it is well to have them in the same volume with the biology, for this arrangement does tend to relate the two subjects and points up areas where more work should be done in order that one field keep abreast with the other.

Chemotherapy is discussed both from the standpoint of experimentation and from the standpoint of clinical application. Clinicians everywhere will surely find the chapter on clinical chemotherapy extremely useful, but they should also find the chapter on experimental chemotherapy of interest. Likewise, the investigator working with experimental animals will want to read the clinical chapter.

The chapter on clinical investigations is rather sobering. It is recommended that amid the present cry for clinical investigations one read this discussion, which, as the author so aptly points out, "must consist essentially in a listing of difficulties and problems."

The evaluation of diagnostic tests probably takes up more space than is justified. However, the discussions of exfoliative cytology and of isotopes in diagnosis should be of great practical value. Much worth-while information can be found in the chapters on environmental cancer and on statistical studies.

For the research man, *The Physiopathology of Cancer* should prove to be a valuable reference book. The full list of references with each chapter should make it particularly useful. The physician will find much practical information in the clinical and applied sections, and it is hoped that he will find time to read the remainder of the book to gain a fuller concept of the disease with which he is dealing. It is somewhat doubtful that the volume will come up to expectations as a textbook for students. If it does not prove to fill this need, it may be that, in the course of their training students, one of the editors may wish to condense and coordinate the material here into a volume that will provide a good textbook.

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Frontal Lobes and Schizophrenia. Second lobotomy project of Boston Psychopathic Hospital. Milton Greenblatt and Harry C. Solomon, Eds. Springer, New York, 1953. 425 pp. Illus. $12.50.

This volume, written by more than 30 contributors, reports the effects of prefrontal lobotomy on 116 patients. A unilateral (left or right) operation by the Poppen technique was performed in 42 cases, a bilateral in 39, while 35 patients were subjected to a bimedial operation that (in contrast to the bilateral one) spares the white matter lateral to the trephine openings. A sample of about 30 patients was studied intensively prior to and after the operation and the methods, as well as the results of the psychological, sociological, and physiological studies of this sample, comprise the larger part of the volume.

It is concluded that a high degree of tension present preoperatively is predictive for a favorable outcome, that the lobotomy decreases tension, and that most of the intellectual and emotional changes, as well as changes in social behavior, of the lobotomized patients may be the result of the elimination of the tension.

The large majority of the patients have been suffering from schizophrenia for many years. This selection of the prognostically almost hopeless cases will be applauded by all those who believe that the operation is basically a mutilating procedure and that it should not be undertaken without compelling indications.

The different chapters are written very unevenly and the amount of new material they present varies greatly. The reader is likely to find of particular interest the chapters dealing with the psychological and sociological studies where an attempt is made to quantitate all results of the tests.

The fact that solutions of too many problems were attempted on a small heterogeneous sample is a major weakness of nearly all the data. Many findings are not significant from a statistical point of view, and many others reach statistical significance at such low levels of confidence that their interpretation remains quite uncertain. Furthermore, although most patients were diagnosed as schizophrenics not all of them were so diagnosed, and most statistics disregard this fact. Even though the bias introduced is probably not large, it seems unsound to include data (however small) pertaining to various disorders other than schizophrenia into over-all findings that refer to schizophrenic patients.

Despite these reservations, the reader will find many data of great interest, and the main results emerge rather clearly. Sixty percent of the patients were judged to be markedly or moderately improved following a bimedial operation, 46 percent were so improved after a bilateral procedure, and 28 percent after a unilateral operation. These and other data imply that the bimedial operation, which is less destructive than the bilateral one, is likely to be the operation of choice. On the other hand, the unilateral operation promises on the whole much less favorable results than the standard bilateral lobotomy.

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Communications

Can Scientists Write for the General Public?

I SEE from a news item in the February 5 issue of *Science*, page 179, that a batch of "science writers" have come to the conclusion that "scientists do not write well enough to communicate their work to the general public." A mere unlettered scientist myself, I do not presume to question this bit of crystallized journalistic wisdom, especially since the report offers the comforting assurance that journalists themselves "constitute the best possible (*sic*) link between scientist and layman." The difficulty is only that, lacking the insight of a science writer, I find these unqualified assertions hard to understand.

You note the statement that "scientists do not write well enough." Not some scientists. Not most scientists. Just scientists. Now, where did I get the idea that between Aristotle and Francois Bourliere there have been a few people who could make science and write about it too? Perhaps I have been deceived by hearing that Charles Darwin's books were rather effective in their own day. Maybe I am incapable of imagining how much better the *Introduction to the Study of Experimental Medicine* would be if Claude Bernard had had it ghosted. Possibly I have been misled by the continuing success of Sir Charles Sherrington's literary venture. Or it may be that, as a constant reader of the *Saturday Review*, I am confused by so frequently seeing the reviews of new books by people like Julian Huxley, George Gaylord Simpson, Homer Smith, Ashley Montagu, Caryl Haskins, N. J. Berrill, Marston Bates, and many others who are not supposed to be able to write.

Or it may be that the difficulty lies in my own perverted taste. Frankly, I have a weakness for such wit as may be found in Edgar Anderson's *Plants, Man, and Life*, for such grace as in William Morton Wheeler's *Social Life Among the Insects*, for such charm as in George W. Corner's *Ourselves Unborn*, for such poetic inspiration as in Rachel Carson's *The Sea Around Us*. But I do realize, of course, that no true journalist clutters up his work with wit, grace, charm, poetry, or other nonutilitarian qualities.

But now I begin to wonder if it is I who am wrong after all. Scientists who can write have been turned up at such rapid rate in recent years as to cast substantial doubt on their putative literary shortcomings. The explantion seems to lie in the fact that, just in the past decade, editors have begun to seek out literate scientists. The editors of *Scientific American* have demonstrated conclusively that scientists who can give a good popular account of themselves are no great rarity; and the people behind Pelican books and Mentor books have achieved the same result. I hope no one will argue that *Patterns of Culture* or *Ur of the Chaldees* would be better books if Ruth Benedict and Sir Leonard Woolley had let someone else write them. The whole situation reminds one of the recent demise of the ancient superstition that historians (overlooking an occasional maverick like Trevelyan) cannot write; today the historians are turning out readable books by the book-club-ful.

And why not? Good writing, after all, is just clear thinking. Anyone who can think well enough to make advances in any learned field ought to be able to write about his work. I am, of course, aware that many research papers submitted to scientific journals are, from a literary standpoint, putrid; but usually such essays are scientifically not very fragrant either.

When it comes to giving "the underlying principles and methods of science palpable significance for the nonscientist," the practicing scientist has an inestimable advantage over the journalist. The scientist knows, from his own experience, that science is a process, not an end-result; when he attempts to communicate the spirit of science, he is in a sense writing autobiography. It is not scientists who have foisted on the American public the delusion that science is a body of glittering miracles, or that scientific method is a sort of glorified jukebox—put in enough money and out come the jazzy strains of a dazzling new technique. If American science is to receive the long-continuing support it needs, the public must be given insights into the true nature of science, but mere superficial representations of the finished products. It would, of course, not be fair to imply that journalists cannot penetrate below the surface of research —Ruth Moore's brilliant new book would alone give the lie to such a charge—yet few have done so. "Science writers" do have an essential role to play in reporting the facts of science; but interpreting science is better left to scientists.

And one further point: nothing could be more absurd than to claim in 1954 that scientists "frequently resent the attempts to popularize science." From my own modest experience, I can say that those who essay to write popular science will find in the warm and generous approbation of their colleagues their best reward.

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Received February 12, 1954.

Meter Versus Yard

I BELIEVE we should adopt the metric system. Why? For simplicity's sake!

For our first example, let us look into a classroom at a small boy who is having trouble remembering his units of measurement, for example, inches in feet, feet in yards, yards in rods, rods in miles, cups in quarts, quarts in gallons, gallons in hogsheads, pints in quarts, quarts in pecks, pecks in bushels, ounces in pounds, and pounds in tons.

As we look over the boy's shoulder, we notice a great lack of uniformity in the measurements, such as, 12 inches in a foot, 3 feet in a yard, and 16½ feet in a rod, and 320 rods in a mile. We can understand this lack of uniformity if we investigate the origins of some of these measurements. For instance, the first foot was the length of the king's foot; the first official acre was the amount of land a man could plow in 1 day; the first yard was the distance from the king's thumb to the tip of his nose; and, to top it off, the first official rod was the length of the left feet of 16 men lined up to go to church on Sunday!

On the other hand, let us look at a small boy in a country that uses the metric system. As we look over his shoulder, we notice a striking resemblance in the different types of measurements, such as length, mass, and volume. In all types, the words *micro-, centi-, milli-,* and so forth, are used. These few words (only about a dozen) are all that a person must remember when using the metric system. Furthermore, all units of measurement are based on units of 10. This speeds calculation.

Modern science has shifted almost entirely to the metric system. Many large industries have also adopted the metric system because it saves time and labor. What about the rest of us now?

Let's not let an inferior system stand in the path of progress. Let's all get on the metric bandwagon. And for the sake of our country, as well as for ourselves, demand that we have this better system.

Let us all throw the English system out;
The metric method wins without a doubt!

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Received November 2, 1953.

In this issue devoted mainly to book reviews and several articles on the art of writing, it seems appropriate to include this short essay by Dennis Henkel, a 10-year-old boy, who became interested in science at the age of 8 and, according to one of his teachers, has read widely on "nuclear physics, general physics, and many other fields of science" and at the present time "thinks he would like to go into the field of theoretical physics."

Book Review Index

Adventures in Physiology. Henry Hallett Dale, p. 562.
Aim and Structure of Physical Theory, The. Pierre Duhem, p. 517.
Anatomy of the Migratory Locust, The. F. O. Albrecht, p. 561.
Astronomical Photoelectric Photometry. Frank Bradshaw Wood, Ed., p. 548.
Catalogue of Fossil Cirripedia in the Department of Geology (British Museum). Thomas Henry Withers, p. 557.
Chemistry of the Lanthanons. R. C. Vickery, p. 554.
Climatic Change. Harlow Shapley, Ed., p. 546.
Conquest of Plague, The. L. Fabian Hirst, p. 563.
Dialogue Concerning the Two Chief World Systems—Ptolemaic & Copernican. Galileo Galilei, p. 546.
Dialogue on the Great World Systems. Galileo Galilei, p. 546.
Dislocations and Plastic Flow in Crystals. A. H. Cottrell, p. 551.
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Scientific Papers Presented to Max Born. Sir Edward Appleton *et al.*, p. 550.
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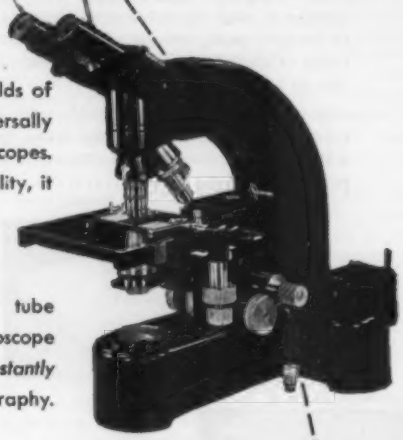

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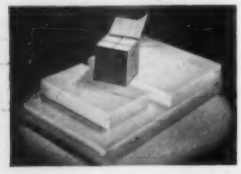

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SCIENCE

The Scientific Author as I Have Known Him

He is a happy man whose work has brought him association with the men and women of science rather than with, say, "personalities" of the entertainment world or politics, or the stuffed shirts of pseudo-Big Business. It is not only that the scientist brings to his work an exceptional competence and a singular assiduity; not only that his work probably does more for human welfare, in the long run, than any other; it is also that science seems to bring to its devotees integrity of mind and purpose and a refreshing modesty.

This rises, no doubt, from the character of the work itself. As each new exploration of a frontier enlarges the understanding, it also uncovers new vistas of ignorance. So there are few prima donnas in science. The atmosphere is rather that of a healthy humility, and few can breathe it without learning tolerance and the agreeable ability to grasp another's point of view. Although I am not a scientist myself, it has been my lot to have had much to do with scientists; and I count the privilege a great one.

I am speaking here, however, not to the point of the scientist *qua* scientist but of the scientist as an author. And first, one must observe that authorship is an inevitable part of the scientific job. Charles Darwin bemoaned the fact that he had to record and report as well as observe, but record and report he did. And every other scientist worth his salt must do the same. Writing is not a dainty accomplishment, like playing the guitar, which the scientist may or may not elect to do. Reporting is an integral and inescapable factor in any research, and no research is complete without the record.

It behooves the scientist, therefore, to make conscious effort to achieve a lucid and impeccable style—and many writers of science material have done this, either intuitively or deliberately. I could name names but the chance of important omission, which could seem invidious, is too great. Yet it has seemed to me that, over the course of the 30-odd years that have constituted my acquaintance with science writers and writing, the quality of that writing has steadily deteriorated. This and what follows is, of course, a generality that ignores notable exceptions.

In the first place, it is much more difficult nowadays to get the scientist to write than it was 30 years ago. He is absorbed and preoccupied with his work, he devotes enormous time and energy to it, and the sheer labor of writing about it in addition apparently appalls him—he has no time for it and no taste for it if the time availed. Compelled at last to put pen to paper, he discovers (or, more accurately, his reader does) that he has lost or never acquired the necessary techniques, which means that, before the manuscript can be printed somebody must patch it.

It is almost axiomatic in a publishing office that nothing conclusive can be learned about an author's style and diction from his reprints. The reprints may have had very much, much, little, or very little of the soothing ministrations of an editor. An original manuscript is the only safe guide.

Whether anything can or should be done to alleviate this situation, I really do not know. Since the men and women of science are certainly not too stupid to learn the elements of English, a remedy could readily be found in more time spent on writing in science courses or, perhaps more to the point, in a general recognition of the fact that sound English is as much a part of training for science as for any other appropriate technique.

But this is, possibly, a millenial hope. We shall probably have to be content, the tempo and temper of the times being what they are, with ramshackle literary edifices, shored up to keep them from falling to pieces and patched and gilded here and there to make them presentable.

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TRAVEL ARRANGEMENTS FOR THE AAAS BERKELEY MEETING

December 26-31, 1954

In time or in cost, a trip from an eastern city to California is not much more than a **round trip** to a midwestern city. Californians who for years have been attending meetings in the East have told their colleagues that the continental distance is the **same** each way, and that it should be the turn of the Easterners to visit the Pacific Coast.

The Association is planning ways it may assist those who will attend the 121st AAAS Meeting on the campus of the University of California at Berkeley, this December. The possibilities include:

1. Low cost AAAS limousines from Oakland and San Francisco airports and railroad terminals direct to the dormitory or hotel of each delegate.
2. Arrangements for traveling together in AAAS cars on fast trains leaving Chicago, Washington, D. C., and New York.
3. Arrangements for chartering first class DC6, 6B, or 7 planes of scheduled airlines—at prices comparable with air coach travel.

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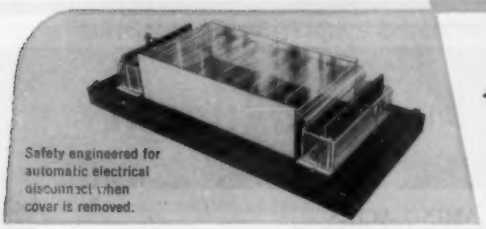

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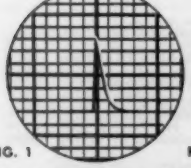

FIG. 1

FIG. 2

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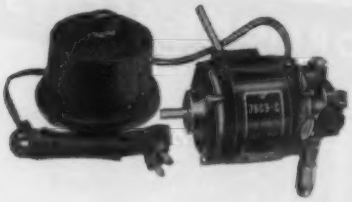

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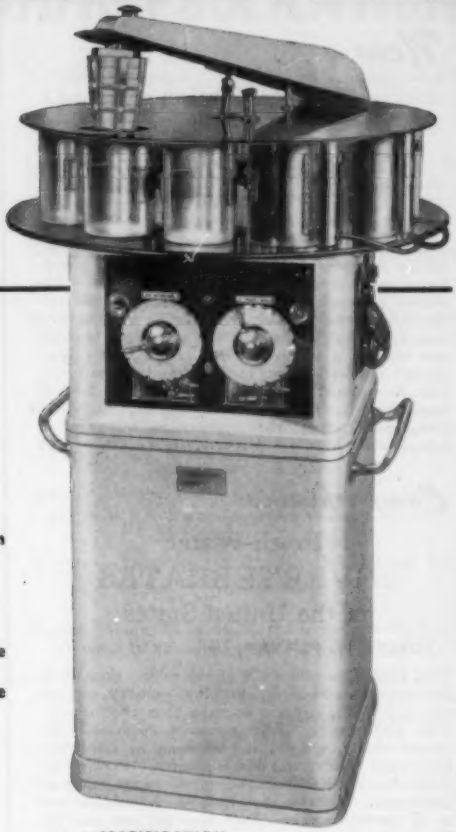

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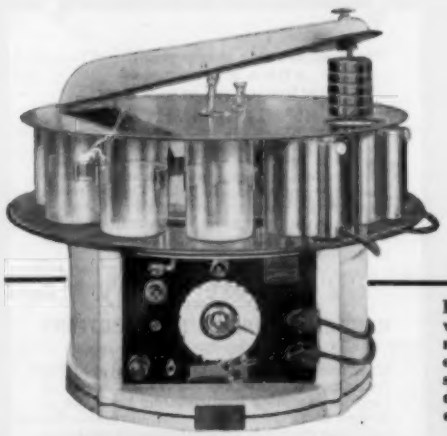

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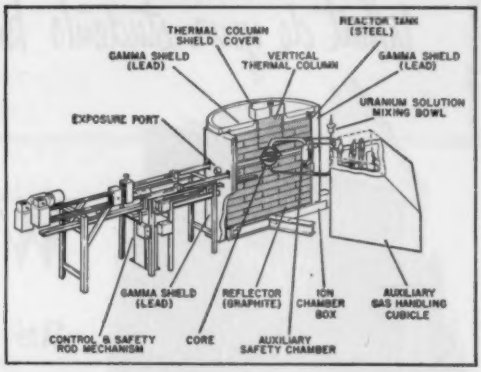

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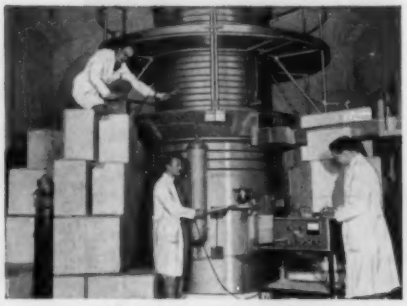

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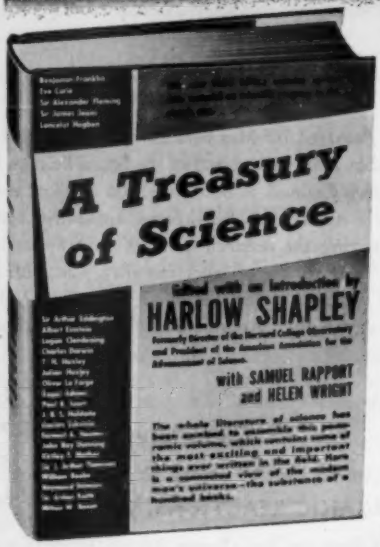

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17–20. Special Libraries Assoc., annual, Cincinnati, Ohio. (A. O. Hanson, Cleveland Public Library, Cleveland 14, Ohio.)

17–21. National Tuberculosis Assoc. and American Trudeau Soc., Atlantic City, N.J. (E. Lovell, NTA, 1790 Broadway, New York 19.)

18–21. American Planning and Civic Assoc., annual, Columbus, Ohio. (Miss Harlean James, 901 Union Trust Bldg., Washington 5, D.C.)

21–26. American Assoc. for the Advancement of Science, Pacific Division, Pullman, Wash. (R. C. Miller, California Acad. of Science, Golden Gate Park, San Francisco 18.)

21–22. Operations Research Soc., 2nd annual, Chicago, Ill. (T. E. Caywood, 203 N. Wabash Ave., Chicago 1.)

24–26. National Telemetering Conf., Chicago, Ill. (W. J. Mayo-Wells, Applied Physics Laboratory, Silver Spring, Md.)

24–27. Symposium on Instrumentation for Industrial Hygiene, Ann Arbor, Mich. (Director, Continued Education, School of Public Health, Univ. of Michigan, Ann Arbor.)

24–28. American Assoc. of Cereal Chemists, annual, Denver, Colo. (C. L. Brooke, Merck & Co., Rahway, N.J.)

29–5. International Ornithological Cong., 11th, Basel, Switzerland. (Jardin Zoologique, Basel.)

29–6. International Medico-Surgical Reunion, 2nd, Turin, Italy. (A. M. Dogliotti, Palazzo delle Esposizioni al Valentino, Turin.)

30–2. International Anatomical Nomenclature Committee, London, Eng. (T. B. Johnston, Guy's Hospital, London, SE 1.)

30–6. International Cong. of Agricultural and Food Industries, 10th, Madrid, Spain. (Sec., 3, Zurbane, Madrid.)

June

1–2. International Neurologic Reunion, 19th, Paris, France. (J. Sigwald, 68, Boulevard de Courcelles, Paris 17.)

4–5. American Psychopathological Assoc., New York City. (J. Zubin, 722 W. 168 St., New York 32.)

6–11. Conf. on Industrial Research, Harriman, N.Y. (R. T. Livingston, 409 Engineering Bldg., Columbia Univ., New York 27.)

7–10. National Plastics Exposition and Technical Conf., Cleveland, Ohio. (Soc. of Plastics Industry, 295 Madison Ave., New York 17.)

7–12. International Cong. of Psychology, 14th, Montreal, Can. (H. S. Langfeld, Eno Hall, Princeton Univ., Princeton, N.J.)

8–10. National Soc. of Professional Engineers, annual, Milwaukee, Wis. (P. H. Robbins, 1121 15 St. NW, Washington 5, D.C.)

9–13. American Soc. for Quality Control, 8th, St. Louis, Mo. (D. Shainin, 70 E. 45 St., New York.)

11–17. Pan American Assoc. of Ophthalmology, 3rd, São Paulo, Brazil. (M. E. Alvaro, Consolacao 1151, São Paulo.)

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